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Philosophy of Science

VOL. 5 October, 1938 NO. 4

Science is *Sciencing*

BY

LESLIE A. WHITE

SCIENCE is not merely a collection of facts and formulas. It is preëminently a way of dealing with experience. The word may be appropriately used as a verb: one *sciences*, i.e., deals with experience according to certain assumptions and with certain techniques. Science is one of two basic ways of dealing with experience. The other is art. And this word, too, may appropriately be used as a verb; one may *art* as well as science. The purpose of science and art is one: to render experience intelligible, i.e., to assist man to adjust himself to his environment in order that he may live. But although working toward the same goal, science and art approach it from opposite directions. Science deals with particulars in terms of universals: Uncle Tom disappears in the mass of Negro slaves. Art deals with universals in terms of particulars: the whole gamut of Negro slavery confronts us in the person of Uncle Tom. Art and science thus grasp a common experience, or reality, by opposite but inseparable poles.

To use the word science as a noun is not, however, without justification. The words chemistry, physiology, history, sociology, etc., are both legitimate and useful. As categories they are derived from two sources. On the one hand, they reflect analytical distinctions which may be made within the field of reality: erosion, respiration, hysteria, voting, etc., are phases or

segments of experience which find their reflections in the categories geology, physiology, psychology, and political science, respectively. On the other hand, division of labor in society, essential in modern times, also finds its reflection in the same, or similar, categories. This is a fact often ignored. Psychology is a category that is a reflection of the division of society into disparate occupational groups just as truly as it is an expression of analytical distinctions which may be made in experience ("subject matter") itself. "Psychology is what *psychologists* (i.e., a guild of workers bearing the label 'psychologist') do," is as valid a definition as "psychology is the study of mind, or behavior." The one is an expression of social reality, of social derivation; the other derives from the nature of the subject matter of the study.

This dual nature of the categories becomes manifest in the recurrent protest against the partition of science into "watertight" compartments, in the impossibility of telling whether a given study is historical, sociological, or psychological. Does the story of John Brown's "insurrection" belong to psychology, sociology, economics, political science, or history? Obviously to all. Nor can the distinction between inorganic, organic and superorganic (or cultural, humanly social) withstand the categorizing process which is implicit in the division of labor in society. When Harlow Shapley studies the responses of ants to varying quantities of heat reaching them from the sun,[1] is he an astronomer or an entomologist? "Astronomy is what an astronomer does as an astronomer," whether he be studying comets, ants or the growth of trees.[2] The biologist in a certain

[1] "Thermokinetics of *Liometopum apiculatum Mayr*," and "Note on the Thermokinetics of Dolichoderine Ants," *Proc. National Academy of Sciences*, VI (1920), 204–211; and X (1924), 436–439, respectively. He found a close correlation between the speed of ants and temperature, within certain temperature limits.

[2] Dr. A. E. Douglass, an astronomer at Steward Observatory, University of Arizona, has, by working out a correlation between climate and growth of trees, provided archaeologists with the most precise method for dating prehistoric remains unaccompanied by written records that has ever been devised. In this instance the astronomer has become an archaeologist. See "The Secret of the Southwest Solved by Talkative Tree Rings," *National Geographic Magazine*, December, 1929.

capacity must be geologist just as the geologist must, at times, turn biologist.

The custom of viewing "science" as a vast terrain divided into a number of "fields" each tilled by its own appropriately named guild has a certain justification in utility and convenience. But it tends to obscure the nature of science as a way of interpreting reality, to spread confusion in the ranks of scientists and laymen alike. The use of the word science as a noun not only leads to jurisdictional disputes—does the study of juvenile delinquency belong to sociology or to psychology? the study of fossils to geology or to biology?—but to such questions as, is history a science? is sociology a science? There is a tendency to identify "science" with some of its techniques. For example, one can perform experiments in chemistry and make accurate predictions in astronomy. Chemistry and astronomy are "sciences." Experimentation is exceedingly limited in sociology and predictions in history are seldom more than guesses. Therefore, the tendency is to say, "history and sociology are not sciences." Despite the fact that much of geology is more historical than certain studies of human culture, there is a willingness to call the one "a science" but to deny this status to the other.

Then a distinction is made between the physical sciences (frequently called by the flattering term "the exact sciences") and the "social sciences." Implicit in this distinction is the assumption that a fundamental difference obtains between the nature of physical reality and human social reality. This assumption leads to, if indeed it does not include by implication, the further assumption that the data of human society being essentially different from the data of physics ("the exact sciences") are really not susceptible to scientific treatment, hence the social sciences are really not sciences at all;[3] they are not and cannot be "scientific." The same observations are made, although with less emphasis, with reference to biology: "Biology

[3] "... I think that social science is like a Welsh rabbit—not really a rabbit at all." A. E. Hooton, Professor of Anthropology at Harvard University, in his *Apes, Men, and Morons*, p. 62.

is less scientific than physics, but more scientific than sociology." These assumptions are not only confusing; they are unwarranted. The basic assumptions and techniques which comprise the scientific way of interpreting reality are applicable equally to all of its phases, to the human-social, or cultural, as well as to the biological and the physical. This means that we must cease viewing science as an entity which is divisible into a number of qualitatively different parts: some wholly scientific (the "exact sciences"), some quasi-scientific, and some only pseudo-scientific. We must cease identifying science with one or another of its techniques, such as experimentation. We must, in short, view science as a way of behaving, as a way of interpreting reality, rather than as an entity in itself, as a segment of that reality.

Experience is the interaction of the organism and its environment.[4] Experience, or reality,[5] consists then of a multiplicity of varied interactions. Conceptually we may view reality as a one, interaction constituting a continuum of experience. Reality is analyzed on a perceptual level; it is divided into odors, sounds, tastes, etc. Conceptually, experience is analyzed with symbolic instruments—words, mathematical symbols, etc. Matter, energy, time, space, motion, etc., are conceptual devices with which we analyze reality and in terms of which we make our adjustments to it. Matter, energy, time, motion, and so on are not therefore discrete entities, but aspects or phases of a common reality. Similarly we may analyze the totality of reality, insofar

[4] The intra-organic functioning of the organism might also be included here. But since no organism exists except in an extra-organic setting, and since intra-organic functioning has significance only in terms of the interaction of organism and environment, we may confine ourselves to the latter and employ the following equation: organism x environment = experience.

[5] I shall use the term reality as synonymous with experience thus defined. This is quite unwarranted from a philosophic point of view: we can be sure that the whole of reality is not experienced by us. But from an anthropological or behavioristic point of view, the term "reality" may be justified, since any portion of reality lying outside and beyond the range of experience is not significant behavioristically. From the point of view of behavior, then, that is to say, within the scope of science (sciencing) experience and reality are coextensive and equivalent, and hence may be regarded as synonymous. The term reality is to be preferred to experience because of its philosophic connotation. Reality is our ultimate interest.

as we can experience it, into equivalent component parts, or "units," which we may call events. Experience is therefore conceived by us on the one hand as a one, as a totality, and on the other as an infinite number of parts, or events.

"Whole and parts" means relationships. "Relationship," too, is another conceptual device, a symbolic instrument, with the aid of which we render experience intelligible to a degree, and by means of which we effect our adjustments to our environment. Events are related to each other. But how?

"Every event that happens in the world is determined by the space-coördinates x, y, z and the time-coördinate t."[6] The fundamental relationship, or "interval," between events is one of space-time. Whereas formerly space and time were thought of as properties of the external world independent of each other, they are now seen to be merely *aspects* of the basic and primary property, space-time. To quote Minkowski's famed phrase: "The views of space and time which I wish to lay before you have sprung from the soil of experimental physics, and therein lies their strength. They are radical. Henceforth space by itself, and time by itself, are doomed to fade away into mere shadows, and only a kind of union of the two will preserve an independent reality."[7] Thus reality confronts us, in modern thought, as a four-dimensional continuum; the process of reality in which events are manifested is a temporal-spatial (or temporal-formal) one.

Thus the primary and fundamental relationship between events is temporal-formal. But by purely logical analysis, we may distinguish the temporal aspect of the process from the spatial; although inseparable in actuality, we may occupy ourselves with either to the exclusion of the other. Thus we may distinguish three kinds of processes, one primary, the spatial-formal, and two secondary and derivative, the temporal on the one hand and the spatial, or formal, on the other. In the first category we would

[6] Article *Space-Time*, by Albert Einstein, in *Encyclopedia Britannica*, XXI, 107 (14th ed.).

[7] Address delivered at the 80th Assembly of German Natural Scientists and Physicians at Cologne, Sept. 21, 1908.

deal with events as being related to each other by space-time intervals; as for the other two we would in the one consider the interval (or relationship) in its temporal aspect only; and in the other the interval would be dealt with in its spatial, or formal, aspect alone.

Sciencing must adapt itself to the structure of reality; its tools must be so shaped and its techniques so ordered as to grasp reality effectively and render it intelligible, if not controllable, to us. This means, therefore, that we shall have three ways of sciencing: one which grasps the space-time property of reality in its entirety, and two subsidiary and derivative ways, each of which deals with two aspects of this property, viz., space and time respectively. All of "science" or sciencing will be found to be assignable to one or another of these three categories; there is no way of sciencing apart from these three.

"History" is that way of sciencing in which events are dealt with in terms of their temporal relationships alone. Each event is unique. The one thing that history never does is to repeat itself: Lincoln is assassinated only once. To be sure, the events which constitute history are related in ways other than temporal. This must of necessity be true since all kinds of relationships are equally attributes of a common reality. But in "history-ing" we arbitrarily select for our consideration the connective tissue of *time*, and just as arbitrarily ignore the relationship of space.[8]

This process of reducing concrete experience to artificial abstractions, or, to put it more precisely, the act of substituting concepts, "free inventions of the human intellect" (to borrow Einstein's phrase), for the concrete experiences of the senses,[9] is not only unavoidable, it is the very essence of sciencing.

"History," or the temporal aspect of experience, is coextensive with reality; it is a property common to the inorganic, organic, and superorganic (or *cultural*, to use the anthropological term, or

[8] To be sure, those who bear the label "historian" concern themselves with relationships other than temporal: they may wish to know *where* Lincoln was assassinated as well as when. "The temporal process" would probably be a better term for our purpose here than "history."

[9] "The relation is not analogous to that of soup to beef, but rather that of wardrobe number to overcoat," p. 353, "Physics and Reality," by Albert Einstein, *Jour. Franklin Institute*, Vol. 221, No. 3 (March, 1936).

human-social, to use a sociological term) orders of phenomena. Stars, the solar system, the earth, rivers, lead, granite, plants and animals, species and individuals, customs and institutions, each have their respective histories. Astronomy, physics, geology, biology, psychology, sociology, and anthropology are then, in part at least, historical "sciences." There is no antagonism nor even distinction between history and science: history is simply one way of sciencing whether it be in geology or sociology. If we refuse to accept this conclusion then we are forced to its alternative: "an astronomer is a scientist when he deals with a non-temporal, repetitive process, but when he concerns himself with a chronological sequence of events (the history of the solar system, e.g.) then he is no longer a scientist."

Events are related to each other spatially, and we may deal with reality in terms of spatial, or formal, relationships, ignoring the aspect *time*.

Spatial relationships between events may be regarded as either constant or variable. Events, or material objects, whose mutual spatial relationships are regarded as constant, constitute a *structure*. This property is characteristic of all phases of reality. In the inorganic, organic, and superorganic levels it manifests itself in the forms atoms, molecules, stars, constellations, planets, orbits, strata, the elements; in skeletons, bones, muscles, organs, bodies, limbs; in families, clans, societies, grammars, constitutions. When the spatial relationships uniting a number of events, or material objects, are regarded as variable, then we speak of *function*. This property likewise manifests itself on all levels of reality in atomic, molecular, meteorological, astronomic behavior; in physiological and psychological processes; and, on the superorganic level, in cultural processes. Thus the physicist, chemist, astronomer, geologist, zoologist, botanist, physiologist, psychologist, sociologist, linguist, anthropologist, etc., are all directly concerned with the spatial (non-temporal) aspect of reality, in its structural or functional aspects, or both.

We come now to the third kind of relationship, or process: the temporal-spatial.[10] This is like the two preceding processes,

[10] It should be noted that this classification can have, logically, only three categories: t, s, and ts.

but different from each. As we have already noted, all three kinds of relationships are always intrinsic in any series of events, in any phase of reality. The temporal process (or "history") is a selective arrangement of events according to the principle *time*. Spatial relationships, though actually existent in these events, are disregarded: in the history of thought it is immaterial whether Newton cogitates under an apple tree or in his bath. Similarly, when dealing with spatial relationships, i.e., with structure and function, the time relationships which are inseparable from these events in objective reality are here divorced by logical analysis: the structure of the crystal, the rusting of iron, respiration, cowardice, secret societies, are studied without reference to clocks or calendars.

But in the temporal-spatial process both temporal and spatial relationships are simultaneously significant. And it is not a case of time *and* space—"up from the South at break of day... and Sheridan twenty miles away." The conventional historian wishes to know not only that Napoleon fought battles, but where he fought them. The zoologist and the ethnologist are interested in the distribution of species and culture traits as well as their history. These are examples of a simultaneous interest in both temporal and spatial relationships. But they are not examples of temporal-spatial relationships. Hydrogen + oxygen = hydrogen + oxygen; t + s = t + s. But hydrogen x oxygen = water (H_2O); t x s = ts. The temporal-spatial process is not, then, equivalent to a space *and* time organization of phenomena; it is not the *sum* of these factors but their product.

It is of interest to note in passing that in many instances in which both temporal *and* spatial relationships are involved, that one is significant only in terms of the other. Thus the thickness of a geologic stratum measured in feet indicates its age measured in years. Similarly, the distribution of a plant or animal species indicates its age[11]: the more wide the distribution the greater the age. And, using the same principle, the anthropologist has

[11] I.e., it tends to. The correlation between spatial distribution and age is not always close.

been able, in many instances, to reconstruct the history of a tool, myth, custom, or institution by inference from its geographic distribution.[12] And, of course, our clocks measure time by a repetitive movement of a mechanism through space.

The temporal-spatial process is an evolutionary, or developmental process. It is to be distinguished from the temporal process on the one hand and the formal process on the other. Like the others, this process is inherent in all experience and is manifest in all realms of reality, inorganic, organic, and superorganic. Thus we have stellar and cosmic evolution, organic evolution, and cultural evolution. This process differs from the temporal and formal processes in that in the evolutionary process, time and space are both integrally involved, they are fused, inseparable. Evolution is temporal-alteration-of-forms. A comparison of these three processes will make each one more distinct.

The temporal process is non-repetitive. In the sequence or process which is temporal (and temporal only), each event is unique; it occurs only once. The Rocky Mountains are formed only once, there is only one Würm glaciation, each raindrop is unique, each movement of every living creature is distinguished from every other movement,[13] John Brown is executed only once, each meeting of the women's sewing circle is a unique event. The spatial, or functional, process, being non-temporal, is repetitive; mountain systems may be formed repeatedly, ice-age may follow ice-age, raindrops fall again and again, water freezes, ice thaws and water freezes again, metal may be melted and remelted, monkeys sneeze, men die, insurrectionists are executed repeatedly, prices rise and fall and rise again, societies and clubs are organized in every age. The evolutionary process, being in

[12] See A. L. Kroeber's essay, "The Culture-Area and Age-Area Concepts of Clark Wissler," in *Methods in Social Science*, Stuart A. Rice, Ed., Chicago, 1931.

[13] It *may* be. Actually they usually are not, for the reason that such distinctions except in rare instances—such as the real or imagined kick of Mrs. O'Leary's cow that started the great Chicago fire, or the honking of the geese who "saved Rome"—have no significance for *us* as ordinary human beings. But for a philosophy of science the sneeze of an anonymous monkey in the depths of a jungle is as significant as illustrating the uniqueness of each event in a temporal series as is the birth of Christ or the death of Caesar.

part temporal in character, is non-repetitive[14]; a reptile becomes a mammal only once; radium decomposes only once; stars "die" only once.[15] Growth is also a temporal-spatial process; the term, however, is usually applied to individuals rather than to classes. Growth is a non-repetitive process: one is a child only once—second childhood is always a novelty.

Even at the cost of repetition, it might be well, for the sake of clarity, to re-emphasize the nature of the distinctions just made. Actually, each event has a four-dimensional character and has its place in a four-dimensional, space-time continuum. Thus the raindrop is an event in the process of cosmic evolution, and we may view it as such. But we may also view it in other contexts: in a purely temporal context, or in a wholly non-temporal context (in which we consider only the alteration in spatial relationships between the raindrop, the earth, the clouds, etc.). The contexts are, of course, devices of our own making. They are arbitrarily selected points of view from which we regard and consider reality; they are the forms, the channels, so to speak, within which we science.

[14] Actually, this may depend upon one's point of view, or more accurately, upon the temporal scope of one's vision. To us, the cosmic process seems to be evolutionary in character: the universe is expanding (it may be assumed), or matter is being transmuted into energy. The process seems to be temporal-formal in character: non-repetitive and irreversible. But this appearance may be an illusion due to our infinitesimally brief span of observation. Were it longer, sufficiently longer, the cosmic process might reveal itself as a repetitive one: a period of contraction might follow expansion, and so on, in an endless series of pulsations; matter may be transmuted into energy and re-congealed into matter, an endless vibration of a cosmic pendulum. So, to a creature which, compared with us, had an infinitesimally brief span of observation, the repetitive and rhythmic character of respiration or the heart beat or the rusting of iron would appear to be evolutionary in character, for seeing only a minute part of the process, neither the beginning nor the end, he would observe only a temporal alteration of form, and might declare it to be a non-repetitive process. And he would be correct too, for the *process* which he observes *is* non-repetitive, just as the dying star and the decomposing radium represent non-repetitive processes to us. Thus, whether a process be labelled repetitive or evolutionary depends upon the unit of measurement. Any repetitive process is made up of a sequence of events which in themselves are non-repetitive. Conversely, any repetitive process is but a segment of a larger one which is evolutionary in character.

[15] One must not confuse *duplication* with *repetition*: there may be transitions from reptile to mammal in different races; one stellar death may follow another. These are duplications, not repetitions.

The formal process is reversible as well as repetitive. Water freezes, ice thaws; iron rusts, iron oxide decomposes; hay becomes beef; beef may become hay again; revolt and reaction are cyclical and opposite processes in society; prices rise and fall, etc. But the temporal order of events remains immutable; it cannot be reversed. Only in *Through the Looking Glass* do Queens scream before they prick their fingers, or Alices pass the cake before they cut it. The evolutionary process being temporal as well as formal, is likewise irreversible. The stars do not reabsorb energy once emanated, mammals do not return to reptilianism, the days when knighthood was in flower can never return, "make me a child again just for tonight" is an impossible request.

The historic process and the evolutionary process are alike in being temporal in character, i.e., non-repetitive and irreversible. But, whereas the historic process is merely temporal, the evolutionary process is formal as well: it is a *temporal-sequence-of-forms*. Historically Ely Whitney and the invention of the cotton gin are inseparable events in a chronological sequence. But had Whitney died in his cradle the evolutionary process expressed in technology would have produced a machine for ginning cotton. Similarly, although Lincoln is bound historically to the emancipation of slaves and Darwin to the discovery of certain biological principles, the processes of politico-economic evolution would have achieved the one without Lincoln just as evolution of thought would have produced the other without Darwin. The invention of the calculus, which took place almost simultaneously, and independently, in the activities of Newton and Leibnitz, was the logical expression of a developmental process, i.e., it was the emergence of a new mathematical form from previous forms. Just as the invention of the calculus was not dependent upon either Newton or Leibnitz alone, so it was not necessarily dependent upon them both; it would have occurred eventually if both Newton and Leibnitz had died in infancy. The development of mathematics, like the development of technology or medicine, is an evolutionary process[16]: new forms

[16] Einstein and Infeld have called their recent book The *Evolution* of Physics, not the *History* of Physics, it is significant to note.

grow out of preceding forms. But in whose person and labors a new form is to appear, and when and where it is to appear is a matter that belongs to the context history alone. From the point of view of the evolutionary process every *historical* event is an *accident* and hence unpredictable. We may predict that a cure for cancer will be found, but to predict who will make the discovery and when is impossible. That the nations of Europe will be embroiled again in a great war in the not distant future is as safe a prediction as one could make; the development of technological, economic, political, and military forces makes another war inevitable. But who will strike the spark that will set off the conflagration, and when and where—what archduke or official will be shot, when, where, and by whom—it is utterly impossible to say. The passing of a star, drawing out from the sun a gigantic filament from which the planets of our solar system were formed, if a fact, is an historic fact; the process is an historic process in which specific and severally unique events take place in a purely temporal context. But this is quite a different process from that of cosmic or galactic evolution as exemplified, for instance, by the equi-partition of energy, or the transmutation of matter into energy, the dying of a star. Similarly, in the biologic realm, the narrative of the specific wanderings over the face of the earth, the struggles, intermixture, vicissitudes, etc., of the various species and races of man is quite a different story from the account of their evolutionary development.

So far, we have spoken of the inorganic, organic, and superorganic realms, or levels, of reality as if these distinctions could be taken for granted. For the sake of completeness and clarity, however, a few words on this subject are desirable.

The distinctions between these levels, or strata, of reality are valid, and are fundamental for science. The phenomena of these three levels do not differ from each other in that one is composed of one kind of basic substance, another of a different kind. They differ in the manner in which their component parts are organized into patterns or forms, respectively. Basically all reality may be assumed to be made up of a common

stuff; differences in various manifestations of reality are due to differences in the forms in which reality confronts us. There are classes, or kinds, of forms amid the infinite range of specific variation. Inorganic, organic, and superorganic are labels for three qualitatively different and scientifically significant categories of forms of reality.

The category inorganic includes, of course, those phenomena which we call inanimate, lifeless. "Organic" includes living organisms. The superorganic category is composed of phenomena called by the anthropologist *culture*. Culture is a term which includes such things as tools, clothing, art, social organization, religion, systems of thought, etc. Culture is an organization of extra-organic mechanisms by means of which a particular animal, man, adjusts, and strives to adjust himself to his environment in order to live securely, fully, and comfortably. Culture, in the anthropological sense, is the possession of man and of no other creature. The essential quality of this organization of such diverse phenomena as hats and ethics, amulets and politics, spears and logarithms, is the *symbol*.[17] Human beings alone of all creatures are able to use symbols: that is, they alone are able freely to endow any perceptible phenomenon (be it material object, gesture, color, sound, or odor) with a meaning which cannot be perceived with the senses.[18] Because of this ability human beings alone possess a culture. And it is cultural phenomena which comprise the category of the superorganic.

Even a casual inspection of our three categories reveals the fact that organic phenomena and superorganic phenomena are

[17] Tool-using is found among sub-human creatures, such as apes. But without articulate speech which provides a means both to communicate experience and to endow it with continuity, tool-using cannot become habitual, traditional and, hence, cumulative and progressive. The apes have the beginnings of culture but they never progress beyond beginnings. They make innumerable starts, but never arrive.

[18] When an ape or man uses a random object as a tool he thereby endows that object with meaning—"poke meaning," or "strike meaning." But this endowed meaning is dependent upon the physical properties of the object-tool, and the *meaning* can be perceived with the senses. But when man endows a combination of sounds, or a color, or a gesture, with *symbolic* meaning (e.g., the word *see*, the x in algebra, the color of mourning, the tipping of a hat), the meaning is *not* dependent upon the physical properties of the symbol's *form of expression*, and hence can not be perceived with the senses.

but particular kinds of organization of phenomena in the inorganic, and the organic and inorganic categories, respectively. Thus, a plant or an animal is but a peculiar form of organization of carbon, oxygen, calcium, etc. Likewise a cultural phenomenon is but a manifestation of organic (human beings) and inorganic (inanimate objects) phenomena organized in a special manner. Thus the phenomena of the organic level (for levels, or strata, are what these categories are in reality) can be dealt with in terms of inorganic phenomena: a plant or animal is so much carbon, nitrogen, hydrogen; it has weight, will fall as a rock, may be frozen, transformed by fire, and so on. Similarly, a phenomenon of culture—a man taking a Christian oath of office—may be dealt with in terms of his gestures, vocal and manual, and these, in turn, together with the book upon which the oath is taken, can be treated in terms of acoustics, mechanics, and physical and chemical properties of the Bible. and so on.

But the fact that the phenomena of one category (except the first, the inorganic) can be "reduced" to the one, or ones, below it does not destroy the categories themselves, nor even minimize their distinctness. A projectile, a pterodactyl, a bird, a squirrel, a fish, a bat, and a bee "fly through the air." A physicist could deal with each as a projectile, in terms of mass, energy, atmospheric resistance, etc. To the physicist all of these objects are alike projectiles: the qualitative differences between them do not exist for the physicist. But merely because they do not exist for him does not mean that they cannot and should not have significance for other men of science. That which distinguishes the inanimate projectile from all the rest on the one hand and that which distinguishes bird from bat and fish on the other, escapes the physicist entirely. Likewise the essential feature of the cultural phenomenon in our example above, the Christian oath, escapes the psychologist[19] and the physiologist as well as the chemist and physicist.

[19] It might seem, at first glance, that the psychologist could give a complete account of the swearing of an oath. But a second thought shows that no psychological examination of the human organism and no amount of psychological laws of human behavior would enable one to tell why one person took an oath to Jesus on a Bible and why another took

Thus we see that we have three qualitatively distinct levels or strata of phenomena: the cultural, or superorganic, which is characterized by the symbol; the organic, which is characterized by the cell; and the inorganic, which is characterized by the atom, proton, electron, wave, or whatever other unit, or units, the physicist decides upon. Being distinct, the phenomena of these three categories must be studied on their respective levels.[20] That is, organic phenomena must be studied *as* organic phenomena and not as inorganic phenomena if we wish to understand organisms as organisms. A chemical analysis or a description in terms of electrons will tell us nothing of respiration, sex, or mammalianism. Similarly, in the case of culture, or superorganic phenomena, we must study it *as* superorganic phenomena and not as psychological or biological phenomena if we wish to understand culture *as* culture. Such things as Grimm's law, kinship systems, clans, logarithms, laws of cultural evolution, and so on, lie outside and above the fields of psychology and biology.

There are, however, instances in which our ends are not served by maintaining the distinctions between these three levels. We may wish to inquire into the relationship between one level and another. Inquiries of this sort are, needless to say, as legitimate and potentially profitable as any other. Thus

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an oath to the Sun on a tobacco pipe (Plains Indians), any more than a psychologist could tell by psychology alone whether a given human organism would speak English or Chinese.

[20] It has been only recently that the superorganic level of phenomena has been recognized as a distinct stratum of reality. But even today there are many (anthropologists included) who insist upon dealing, or prefer to deal, with cultural phenomena in psychological or biological terms rather than in terms of its own order of existence. There is a close and interesting correlation between the arrangement of the strata of reality and the history of the sciences. At the bottom lies the inorganic; this was the first to be cultivated with success by the scientist in the persons of astronomers, physicists and chemists. Lying above the inorganic, and being in actuality, merely a special and peculiar form of inorganic components, is the organic. This was next in order of scientific development. Finally, superimposed upon the organic stratum is the superorganic or culture layer. The scientist has only recently invaded this realm, and, so far, his achievements here have been insignificant when compared with his triumphs in lower levels. But, given time, maturity will come here also, and with it achievement.

bio-chemistry inquires into the relationship between the inorganic level and the organic. Similar inquiries are directed to the relationship between the organic category and the superorganic. Take the Oedipus complex of psychoanalysis, for example. Love of a boy for his mother and hostility for his father springs in part from his organism. But it may be derived, or at least conditioned, by the form of the family into which he is born. If the family is patriarchal the expression (if not partial derivation) of the boy's attitudes will be different from those in a society organized matrilineally, or one recognizing both lines of descent equally. Thus the Oedipus or Electra complex, as well as all other examples of *human* behavior—i.e., human behavior as distinguished from non-human, or sub-human, behavior; there is nothing peculiarly *human* about a sneeze, e.g.—are made up of elements drawn from two different categories: the biological and the cultural. The formula for human behavior is: Human behavior = Human organism x Cultural stimuli.

Studies of soil erosion inquire into the relationship between such things as farming or grazing methods, lumber industry, the prices of building materials, and reforestation; the quantity and frequency of rainfall, natural and artificial drainage structures; winds, and legislation. The search for a material that will destroy plant or animal pests may involve relationships between all three levels: the price of commodities, the organic pests and the chemicals capable of killing them.

Here again, whether we deal with reality in terms of distinct categories, or levels, of phenomena, or in terms of relationships between them, depends upon our purposes and ends. Both approaches are equally legitimate and potentially profitable.

In summary, we see that we have two classifications of reality which cut across each other at right angles: the one has to do with structure (the "atom," the cell, the symbol), the other has to do with process (temporal, formal, and temporal-formal). This gives us nine categories in which all reality and all manners of sciencing may be logically and consistently divided as indicated in the accompanying diagram.

	Temporal	Spatial-Temporal	Spatial
Super-organic	"History," Culture history, or History of Civilization	Cultural Evolution	Non-temporal, repetitive, culturally determined processes in human society
Organic	Racial history of Man History of animal and plant species, genera	Organic evolution Growth of individuals	Non-temporal, repetitive processes in organic behavior: intra-organic (physiology), extra-organic (psychology)
Inorganic	History of solar system, of the earth, a continent, mountain system, river, drop of water, a grain of sand	Cosmic, solar, stellar, galactic evolution Disintegration of radio-active substances	Non-temporal, repetitive processes in physics, chemistry, astronomy

On the inorganic level we have cosmic and galactic histories (such as they are or may be), the history of our solar system, the history of the earth or of a continent, a mountain chain, a river, or even a snowflake, encompassed within the purely temporal context. In the purely formal-functional context we have the non-temporal and repetitive, structural and functional aspects of astronomy, geology, chemistry, and physics. And in the primary category, the temporal-formal one, of which the other two are but aspects, we have cosmic,[21] galactic, stellar,[21] and solar evolution, and the decomposition of radio-active substances.

On the organic level, in the purely temporal context, we have the histories of plants and animals, of genera, species, and individuals, both human and non-human. Our greatest concern in this category is, perhaps, with human beings: we are intensely interested in the problems of the origin, diffusion, extinction, intermixture of various species and races of mankind. But a

[21] See, e.g., the discussions on "The Evolution of the Universe" by J. Jeans, R. A. Millikan, Eddington, W. de Sitter, G. Lemaître, et al, in Appendix, Report of the Centenary Meeting, British Assn. for Adv. of Science, held in London in 1931. Also, H. N. Russell's article "Stellar Evolution," in *Encyclopedia Britannica*, 14th ed.

like interest in plants and sub-human animals is not insignificant. In the formal-functional category we have studies of morphology and function; anatomy, the non-temporal, repetitive aspects of physiology and psychology belong here. And, in the temporal-formal category, we have evolution of organic forms in general, of genera, species, varieties in particular. The growth of an individual also comes within this category.

Biography, the history of a human individual, should, in most instances, be regarded as dealing with both the organic and the superorganic levels since our interest in a human individual is seldom, if ever, divorced from the culture in which he has his *human* being. Similarly with significant individuals in the sub-human animal or plant world: the cow that started the Chicago fire, the goose that saved Rome, the wolf who suckled Romulus and Remus, Man o' War, the hemlock that killed Socrates, each is significant only because it enters the context of human cultural history.

On the superorganic level we have culture history; the history of nations, reigns, tribes, institutions, tools, ideas, beliefs, etc., in the temporal context. In the formal-functional context, we have the studies of "social morphology" in sociology, cultural anthropology, and the other "social sciences." The so-called functionalist school of cultural anthropology,[22] the "Chicago school" of sociology,[23] e.g., belong here. In the basic category, that of evolution of culture, we have at present virtually nothing. After a vigorous and bitter struggle the philosophy of evolution conquered on the organic field, but, after a few brief advances it has been routed from the superorganic field. A few giants like Herbert Spencer and Lewis H. Morgan, in the "boom" days of evolutionism in the second half of the nineteenth century were able to occupy the superorganic field for a time. But the anti-evolutionists have regained the field and have held it suc-

[22] I.e., of Bronislaw Malinowski, A. R. Radcliffe-Brown and their respective students.

[23] See *Introduction to Sociology*, by Robert E. Park and Ernest W. Burgess (Chicago, 1921). Among the students of Professors Park and Burgess: E. T. Hiller, *The Strike* (1928); F. M. Thrasher, *The Gang* (1936); A. Blumenthall, *Small-Town Stuff* (1932); N. Nelson, *The Hobo* (1923), etc.

cessfully since the turn of the century. To be sure both Morgan and Spencer committed errors in the *use* of their philosophy, but a mistake made in the use of a tool does not render the tool worthless. But sociologists and anthropologists have repudiated the philosophy of evolutionism along with the errors of some evolutionists: they have poured the baby out with the bath. But the victory of the anti-evolutionists on the superorganic level is only temporary. As social science matures, the basic concept of science and philosophy, that reality is spatial-temporal in character, will win its way on the superorganic level as it has upon the organic and inorganic levels.

It will be noted, of course, that our conventional names for "the sciences" do not readily fit our category system. But this is quite understandable: the terms *physics*, *zoology*, *sociology*, etc. have come into use as science has grown, and this growth has been more or less accidental. The concepts *time* and *space* existed long before it was discovered that *time* and *space* are but aspects of a third thing for which there is no more adequate a name than *space-time*. But the fact that the names of "the sciences" do not correspond to our nine categories in no way invalidates the categories. The maturity of science in any field can be rather accurately gauged by its vocabulary: as "a science" matures it develops its own terminology. This has taken place extensively in the physical and the biological sciences. And such words as *instinct*, *intelligence*, *race*, *society*, are now being found so difficult to use that it is likely they will give way soon to a more effective terminology.

For the scientific worker such terms as *psychology*, *botany*, *chemistry*, etc., will no doubt continue to be useful and satisfactory except in so far as further division of labor and specialization should make new terms necessary. But for the thinker, for the philosopher of science, new technical terms are needed. I shall not presume to supply names for our nine categories. But, since they represent a realistic and logical analysis of the field, it seems likely that as these categories obtrude themselves more and more into systematic thinking, they will eventually receive names.

The scope of each of the three levels of phenomena has interesting implications for philosophy and for human welfare. The category of the inorganic embraces the cosmos in extent; the two other levels are built up, so to speak, out of inorganic phenomena. The scientific exploration and progressive conquest of terrain on the inorganic level will have the greatest significance for philosophy (at least for much of what has been traditionally labelled "philosophy"); it will bring us closest to reality, closest to the "nature of things." Contrasted with the extent of the inorganic realm, both in time and in space, the phenomena of the superorganic appear infinitesimal and insignificant: all of life, to say nothing of human culture, appears as but a slight rash upon the epidermis of a most microscopic and insignificant cosmic speck. But to us, the cultural phenomena are anything but trivial: war and revolution, financial crises, organized crime, unemployment, poverty, and destitution may be of greater moment than the movements of the wily electron or the temperature of Antares. Insignificant in cosmic perspective, the institutions under which we live, labor and suffer are of supreme human importance. The phenomena of the cultural realm have their own distinctive properties and function in accordance with their own laws just as the phenomena of other levels do. It is possible for us to discover, with the viewpoint, assumptions and techniques of science, what these properties and laws are. And once this has been done, it will be possible to control these phenomena and bend them to our desire and will, to a greater extent than is possible in either of the other two levels, for culture is but a human by-product. This is not a plea for the development of one type of science at the expense of, or even *rather than*, another. But at the present time vast progress has already been made in the physical sciences, and even in the biological. Our control over the resources and brute forces of nature is staggering when compared with that of 1776. And we have won a large measure of success and control in the biological (especially the medical) sciences. But there is so little effective knowledge of our cultural world, so much difficulty and so little control that many sober minds are seriously won-

dering if our civilization will survive the present world crisis. It is the profound conviction of the present writer that the phenomena of the cultural level are as susceptible to scientific study as the data of other levels, and that when concerted and sustained scientific attack is made upon problems in this field that the achievements and triumphs will be as great, if not greater, than in the physical and biological domains. A way must be found to divert the resources of science and society in this direction, else, indeed, our triumphs in physics, astronomy, and medicine may well turn out to be hollow ones.

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Arranging the Sciences

II. Another Experiment

BY

WILLIAM MARIAS MALISOFF

SINCE my discussion of an experimental arrangement of the sciences for certain practical objectives, some comment has appeared which makes it necessary to re-emphasize certain points by a further example. There seems to be something like a desire to paralyze the hand that makes arrangements for fear that undue profound metaphysical significance may be seen in a "mere" arrangement. It is the perennial fear of the *a priori*, the fear of a principle being invoked because of its "self-evidence", and then turning traitor on the actual evidence. I invoke no such principle either overtly or covertly.

An arrangement of the sciences should be first of all a *working model.* This is a guarantee of metaphysical neutrality. At the same time, because of its tentative character, it becomes an invitation to find a law (at least in the minimum sense of a summary description) which (1) includes the greatest number of the sciences (2) allows verification either by the inclusion of new sciences or by deductions from the underlying assumptions and (3) allows for correction. "Laws" concerning the organization-so-called of the sciences will differ in no essential way from any other scientific laws and will not be one iota more or less metaphysical. Logical empiricists, take note that in this way your conception of the unity of science is secured but that

it is not necessary to deny completely the theoretical possibility of laws of nature (you do use the phrase, don't you?) which deal with groups of the laws of nature which constitute the special sciences.

In self criticism, I must add that the disc arrangement to be repeated here for the applied sciences, has a very serious rival

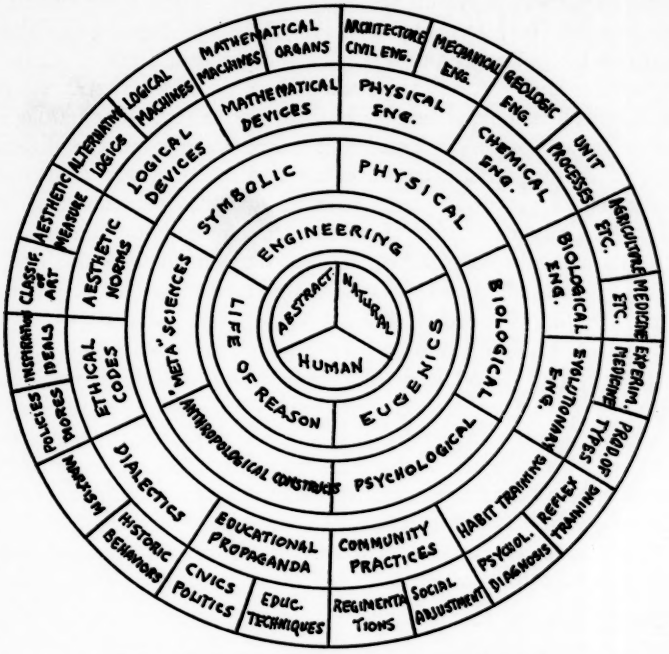

Fig. 1. An Arrangement of the Applied Sciences

in my own thought, namely, the desire to classify the sciences on a scale of increasing postulational complexity. A sincere scientist, however, must give all the weight he can to opposing views. Let the fittest survive, and, perhaps, several approaches will have their uses.

Following the technique of the last communication on this

subject, there are chosen three principal activities of application:—engineering, eugenics and the life of reason. These three terms are to be taken in their broadest senses. My intention is to create blends of the three interests described by me as "abstract", "natural" and "human", without destroying their continuity. Eugenics, thus, may be equated to "animal and human engineering", and the life of reason may be described as "engineering of ideas and ideals". I choose these equivalents in order to make "engineering" the key word, since it suggests application more clearly than the others. The next circle should contain hybrid terms, but since they would represent names for entire groups of *mixed applied sciences*, we decline the difficult task of naming them at present. Yet one can see that there would be a group of cross-sciences which, for example, will stress the elements of architecture in eugenics or the medical aspects in the life of reason, and the like, in great profusion.

The next layer repeats the series of terms used in connection with the pure sciences. Again crossings are possible, but it is more profitable to go on to a ring which names specific applied sciences. Here I venture to name new ones in conformance with my principal intention of using the disc as a device for scouting for new sciences. The game is not ended at this point since one can go on in further rings to name sub-sciences of narrower and narrower scope. Some of these appear in the outermost ring for illustration.

From time to time, I propose to present without prejudice still other arrangements.

New York, N. Y.

Preface to a Theory of Nature

BY

ARTHUR LAPAN

LIKE most other subjects under discussion today, the theory of nature is largely controlled by considerations of knowledge. Treatment of it is, consequently, incidental to the treatment of these other problems, and is undertaken, in the main, because *they* compel it. A brief catalogue of characteristic statements about nature will illustrate this. "Nature," says one writer, "is that which we observe in perception through the senses";[1] and another writes, "It is not experience which is experienced, but nature—stones, plants, animals, diseases, health, temperature, electricity, and so on."[2] Nothing is more apparent in these sentences than the fact that it is not nature that the writers are so much concerned with, as with what it is that appears through the senses. The theory of nature, that is, is not so much guided by an analysis of the word's context and usage, as it is by the question: what is the proper object of knowledge? Consequently it is the latter question that is answered and not the question: what is nature? The reason for this is evident in the purpose of these statements and in their history; their purpose: the desire to do away with the separation between experience and nature, and to transform nature into a field familiar and accessible; and their history: the very framework against which they are directed, for they are embedded in the set-up from which they are trying to disentangle themselves.

[1] Whitehead. *Concept of Nature*, p. 3.
[2] Dewey. *Experience and Nature*, p. 4a.

Perhaps something ought to be said to substantiate the thesis that the sentences cited are controlled by the problem of knowledge, especially since their intent seems to be to escape that problem. In any case, their history should serve to clarify them, and render a different approach to the theory of nature more significant.

As is well known, the attempt to account for the world within which we live, on the basis of nature conceived as a mechanical system, led to shifting the familiar world, in the form of ideas, into the mind. These ideas, the product of the outer world, with the addition, perhaps, of the mind's contribution, were believed to be the proper objects of knowledge; and it consequently became necessary to explain how we can know the physical world when the only things accessible to us are the ideas in our minds. The answer was that these ideas are representative of the world outside. They are the means by which the outer, inaccessible world is known. The mechanical system, then, is not known directly but by intervention, through the medium of ideas. In other words, the familiar world (now mental) was taken to be cognitive and to refer to something beyond itself.

Limitation of the objects of knowledge to ideas in the mind, however, shut the philosophers up within the circle of ideas, and left them powerless to explain knowledge of a world assumed to be beyond it and independent of it. The field of ideas then began to function more and more as the context in the light of which all things were analyzed and rendered intelligible. This was, in truth, the first step towards the doctrine of an 'accessible nature' and its companion, that *nature* is what is perceived through the senses. In a way, Berkeley had taken this step when he distinguished between the real and the imaginary in terms of the coherence and compulsion of ideas, and when he construed representation as a relation between ideas. But Berkeley had not carried this procedure through; instead of a further analysis of the field of ideas, he was led to account for certain of their characteristics, their orderliness, their continued existence, and for their very existence, on the basis of a transcendent deity. He thus forsook the realm of the accessible, conceived by him to be a

mental realm, to be sure, for the inaccessible, i.e. God. In this respect there is but little difference between him and Locke, except a difference in substance; but the functions these substances (God and matter) were called upon to serve were the same. The resolution to construe *all* things in terms of ideas had, however, a different effect. It resulted in the theory that nature is what is experienced, and that nature is accessible.

This transition was due to being limited to the field of ideas, thus provoking the question: how are these ideas to be identified? The identification was made in terms of what we think and feel *precisely* as we think and feel it. Although 'thoughts and feelings' in this identification were originally taken to be mental, nevertheless analysis of their content disclosed that they were experienced as *physical.* Since, however, rigid limitation to the field of ideas, and the consequent elimination of the external world, had necessitated the view that what reality there is is precisely what is experienced, perception was regarded as directly cognitive of that reality. In perception, however, the objects are disclosed as independent and physical. Consequently it followed that what is experienced is *really* independent and physical. This amounted to saying that "nature is that which we observe in perception through the senses," and that "it is not experience which is experienced, but nature. . . ." Thus nature was transformed into a field directly accessible to perception and was taken to be characterized by precisely those traits which perception discloses.

Such a doctrine, however, defined not so much *nature* as what is perceived through the senses; the theory of nature being dependent on the theory of knowledge, while the latter remained preliminary to the former. The inquiry into nature did not proceed independently and unencumbered by the inquiry into knowledge, but was diverted into new epistemological controversies. It may be questioned, however, if it would not be more profitable for the theory of knowledge to *begin* with an investigation into nature, and thence proceed to a definition of knowledge; for this is how the systems of statements we call knowledge were acquired, and *their* acquisition, at least, pro-

ceeded quite independently of any inquiry into epistemology (in the usual sense). Moreover, the definition of nature as what is perceived is not clear. It does not tell us how to distinguish what is perceived through the senses from that which is not so perceived (as in dreams), and also, it fails to differentiate what is meant by the word 'nature,' as it is used in contrast to art, from nature as it is used to include art. The first ambiguity is a serious one since it presupposes what it intends to define—namely, nature—for if we attempt to distinguish what is gotten through the senses from what is not so gotten, we will be compelled to do so in terms of the process of perception. What is gotten through the senses, we will then have to say, has its origin in the world about us, and arises after contact between selected portions of that world and our sense organs. What is not so gotten has its origins in ourselves, in motions of our cerebral centers or memories. In other words, the distinction will be in terms of origins. What is this else, than to say that perception through the senses is perception which has nature both as origin and object, whereas in perception which is not through the senses nature is neither origin nor object? To define nature, therefore, through perception is to define nature in terms of itself. Furthermore, such a definition raises all the old perplexities: is the object of perception the same as its origin, and if not, how can we pass from the one to the other? We thus go, by an inevitable movement, from the attempt to disentangle ourselves from the problem of knowledge back into it again.

The reason is that the field of nature is determined not by a consideration of the word's context and usage, but by an antecedent constraint. It is the *procedure* that is wrong. The conception of nature is made to suit a preëxistent hypothesis, and not the things we call 'natural,' or identify as belonging to nature. Our conclusions consequently become arbitrary and in many cases unverifiable, often putting a stop to inquiry rather than advancing it. The correct procedure would be to enumerate the various uses of the word 'nature' and give examples of it, and thence to proceed to discernment of the reasons in virtue of which the word is applied to the things it signifies, thus disclosing

a definition of it. Such a procedure is wholly analytic, untrammlled by any theory of perception, the evidence for the conclusion resting entirely upon examined subject matter and not upon a compelling history or looked-for result.

When the field of nature is approached through an analysis of the word's usages and their contexts, it must be recognized that, not a theory of perception is controlling, but the principle of causality. The distinction, that is, between what is nature and what is not nature, between nature and art, has to be made in terms of different kinds of causes. This will be shown by the analyses indicated.

The word 'nature' is used as a substantive noun, a subject of attributes that is not the attribute of any subject. In this sense it is used collectively, what is said of the whole not needing to be said of any of the individuals that compose it, as in the statement: nature is self-existent. The word may also be used distributively as the subject of attributes and the attribute of subjects. If we say that the nature of a circle is that it is a plane figure every point of which is equidistant from a fixed point, the nature is the set of attributes which is the necessary and sufficient condition for any concrete thing to be a thing of that type. Nature, here, is the definition or structure of a thing from which its properties and characteristic actions can be deduced. The set of attributes which are taken to constitute the thing's nature may, however, also be the subject of attributes, as when a good or beautiful nature is referred to.

Nature as a subject of attributes only, is used both collectively and as an ontological substantive. The analysis will not be concerned with a definition of nature in the latter sense, since such a substantive, even if it existed, could not be defined, its character and status being taken as independent of everything else. As an ontological substantive Nature is regarded as independent and separable from the things which constitute it. The distinction between nature and its constituents is then conceived as similar to the difference between a container and what it contains, with this addition, that the container is the condition of what it contains. Thus people say: Nature does this and that; or they refer

to the powers of nature and its possessions. Here nature is taken to stamp its own peculiar character on every part of it in the same way that a container may be supposed to stamp its character on its contents, when these contents are taken to depend on the container. The existence and character of the parts of nature become, then, dependent upon nature itself; and the coherence of these parts upon its constitution. In conformity with this alleged constitution, and by its activities, do natural phenomena acquire a stable structure. Also, it is absolutely unique and self-subsistent, the condition of everything in it, itself unconditioned. It cannot, therefore, be understood in the light of anything else, and is consequently indefinable. Such a substantive, moreover, neither is identifiable nor does it serve any use in inquiry. It cannot be discerned in perception or emotional experience, nor is it necessary to account for the objects of perception or the traits of emotions. It is not used in discovering the laws of nature, nor can any of these laws be deduced from it. In fact, it is wholly superfluous; and it is as unintelligible to separate nature from its parts as to separate an army from the totality of its regiments and soldiers. Neither exists independently of its parts, or creates them, or acts as the condition of their structure and order.

Nature, then, is simply the collection of these parts, the field of this order, and would not exist were it not for their existence. What, then, is this nature? We may say that it is the field or collection of natural entities. The identification of the field of nature, that is, must be made in terms of the use of such words as 'natural,' adjectives that are employed to qualify other terms like 'process,' 'state,' 'phenomena,' 'operation,' 'activities,' etc.; and when the identification is made in this way, the noun 'nature' is a collective name referring to these natural somethings, whatever they are taken to be. Nature, then, is the field of natural entities. The problem becomes: just what precisely does the adjective 'natural' distinguish?

The natural is used to designate the lawful or recurrent, as when we speak of a thing as natural if we can explain it by some law. Thus we speak of natural law and of things happening

naturally, meaning thereby stably, customarily, without accident or surprise. It would be unnatural, for example, for a man to beget a horse, or for a saint to turn gangster. The word has also taken on an honorific flavor, as when people refer to unnatural appetites or actions. Also the natural is used over against the supernatural, the miraculous. As so employed it is distinguished from the other usages of the term 'natural,' in that it is then a controversial word, a word over which men fight, and which is used to intimidate or convert them. This indicates when closely examined that the difference between the natural and supernatural designates, not so much a difference between two kinds of events, as it does a difference in their manner of explanation. If the sun actually stood still in the story of Joshua, the natural or supernatural character of the event would not alter the *fact* that it stood still; but the *significance* would be altered. The distinction is not a distinction in fact, but a distinction in interpretation. It is a quarrel over how facts are to be explained, and not over what they are. This is quite different from other uses of the adjective 'natural,' as when for example it is used over against the artificial. In this case the distinction is one in fact, in productivity, and not in explanation. Animals, stars, sexual passions and the like, are natural, belong to nature; tables, chairs and houses are artificial, belong to art. The difference between them does not depend upon how we interpret them. It is a difference with which we begin, and which has itself to be explained. It is something which is not exclusive, like two rival theories jostling for acceptance; but both realms exist side by side without expelling each other. An anecdote, if such be needed, illustrates this primordial difference. A young man encountered an old friend and begins to tell him of an engagement he had had with a young lady known to both of them. "I was with so-and-so last night," he said, "and naturally, she was very beautiful." "Naturally?" queried the friend. "No—artificially."

The distinction between the natural and supernatural then is a distinction in kinds of explanation. In what precisely does this distinction consist? Not in the fact that things natural are wholly accountable, whereas things supernatural are not, since

both attempt to explain. Those who believe in the supernatural will tell you that the event is due to God or demons or angels, thereby assigning a cause to it. The fact that a thing is said to have a cause is therefore not sufficient to class it as a part of nature, since supernatural accounts are frequently scientific in aim and are inspired by the desire to understand, foresee, and control. The use of magic and prayer have these clearly in mind, just as they also may seek to assuage grief or atone for sin, or express surrender and adoration of what is taken to be divine. It is no accident that magic and experimental science for a long time were indistinguishable, and that the one only with great labor extricated itself from the ideas and suppositions of the other.

In one sense, therefore, the supernatural does not designate what is irrational. In another sense, however, it may, as when we say that one thing is the cause of another, but cannot say *how* it is the cause, when the manner of operation not only is unexplained but inexplicable. Thus an incantation may be said to be responsible for disease. But how? Here no explanation can be offered; indeed, it is taken to be mysterious. Hence the first definition of the supernatural: a thing is said to be supernatural when it has a cause assigned to it, but when the operation of the cause is unintelligible. In essence, such an event is truly irrational, since if it were rational, it could be explained in the light of a system of events with which it is logically bound. What, then, is the meaning of the 'supernatural' as inexplicable? It is this, that a supernatural event is one which is taken to be logically unconnected with a system of events. It is said to have a cause, but both it and its cause have no context.

Nature, consequently, is a collective name for events which have a cause and are logically bound together; or, differently stated, natural events are parts of systems. The question whether each event is logically bound to *every* other event, or only to some, that is, whether nature is one system of interconnected events, or whether many systems may be discriminated within it, is not relevant to the present distinction. The important thing is that every natural event is said to have a cause and to be part

of a system of events. The proposition which states the event is, when the event is known, coherent with a network of propositions. Knowledge, as given in propositions, is simply an articulation of the order of nature; and this order is found to be a rational order in which events imply (that is, are articulated by propositions which strictly imply) one another.

Some of the other uses of the words 'natural' and 'nature' are simply species of this meaning or intimately related with it, e.g. the natural as the lawful or recurrent. A thing is lawfully connected with another only because it is logically connected with it. There is no law without a reason. This is exemplified in the procedure of science where statements of law are verified by hypotheses concerning reasons. The propositions stating the recurrence of the tides, for example, are coherent with the propositions stating the relation between tides and the movements of certain heavenly bodies; and these, again, are coherent with and find their reason in the law of gravitation. Recurrences, then, are parts of systems, and exploration of their reasons leads to the discovery of a system of which they are a part. The natural as lawful is, therefore, not intelligible except in the light of the natural as logical. In the process of knowledge the one is an extension of the other, the ground of the other, and the verification of the other. It is an extension of the other because, when we have knowledge of a recurrence, we seek to replace it with knowledge of its reason. In this sense, scientific knowledge is an elaboration of opinion, where, by opinion, is meant knowledge of correlations without knowledge of their causes, and where, by scientific knowledge, is meant the statement of the reasons for a thing, for its being as it is and not differently. The natural as the logical is the ground of the natural as the recurrent, for from the reason we demonstrate the recurrence; and it is also its verification, for unless we have the reason we cannot be secure that we have a correlation at all. It may merely be coincidental and the product of limited and inadequate experience.

Nor is the distributive use of the word 'nature' as definition or structure incompatible with the natural as logical. The nature of the thing is simply its definition, from which the state-

ments of its characteristics can be deduced. In one sense, therefore, the statement of a thing's nature is the reason for its properties. On the other hand, since the definition of a thing's nature is largely a matter of choice, it would be more accurate to say that its nature *is* the logical order of its characteristics. The difficulty that in a demonstrative system the definition may become a theorem and the theorem a definition, if the system is somewhat differently constructed, would thus be circumvented. Systems so ordered would be structurely identical, and it is this identity in structure of theorems and axioms that constitutes its definition, its nature, as it were. It is obvious, then, that the natural as logical, and nature as definition, are two aspects of one thing. The natural is whatever is part of a logical system of events; the nature of a thing is the system of events logically ordered.

Finally there is the natural as it is used over against the accidental and surprising. Whatever intrudes upon a stable order of events is said to be accidental. Shall it then be inferred that the accidental and supernatural are one, as unconnected with a system of things? No, for the supernatural is taken to be unrelated to *any* system of events whatever, whereas the accidental is not bound to the specific selected one. The accidental is only relatively unconnected; the supernatural absolutely so. Accidents occur where histories intersect and find their explanation in the histories taken together. Their logical looseness is relevant only to selected histories, single systems, and in the light of something else, can be shown to be logically grounded.

Nature, then, is a field in which specific things fall into some systems and are excluded from others. It is a collection of ordered *systems* as well as a collection of natural entities, just as an army is a collection of regiments as much as it is a collection of soldiers. This it may be in two senses. First, systems may be utterly discontinuous historically, in that the events of the one form no part of the origin or history of the other. Such systems may exist at the same time or overlap, so that parts of them exist at the same time and parts not; or they may exist at wholly different times. On the other hand, systems that do influence

each other may exist at the same time, as one man affecting another; or at different times, as the Greco-Roman civilization affecting our own. In the case of an utter discontinuity, the understanding of the one does not involve an understanding of the other. In the case of partial discontinuity, part of the events are to be understood in terms of both, and part in terms of each singly. Understanding of systems thus compels the conclusion that their conditions are specific and consequently their origins. A statement of their factors excludes all else as irrelevant. Example: if the tides are explained by the law of gravitation, then their account leads to masses and distances and not to social conditions or colors; and also, it is the motion of the *sun* and *moon* to which they are due, and not to some distant sun. Explanation, therefore, rests upon certain factors and excludes others, as in physics upon number and not morality, and within the category affirmed, is limiting in environment for specific events. Explanation of the motion of A leads, on the basis of the laws of motion, to B and C; but thereby, also on the basis of the same law of motion, it excludes all else.

Systems are also discontinuous in that the laws of one may not be deducible from the laws of another. Thus, the laws of psychology cannot be derived from those of physics. This can be proved simply, for where fields present differences in behavior (are defined in terms of such differences) the object of science is to account for the diversity; but the reason for the diversity cannot be found in what is common to both. Hence it must be accidental to the reasons for what is common. Example: the field of psychology is a field in which organisms pursue ends, whereas the field of physics is not. Yet teleological behavior is a *kind* of motion, so that what it has in common with the field of physics can be explained in the light of physics. Its distinctive differences, however, cannot.

In terms of the discontinuities of histories and deductive systems, the accidental is seen to be opposed to the natural only in a limited sense. With reference to its own history and an appropriate system, the accidental event is wholly natural, for the existence of discontinuities points, not to an irrationality within

nature (used in a collective sense), but to a plurality of logical systems composing it. Any event, therefore, is both necessary and accidental according to the context in which it is taken. The use of the word 'natural' as over against the accidental is thus wholly consistent with its wider use as contrasted with the illogical as such. Not the accidental, then, but the supernatural, that which is related to *no* logical system, is the antithesis of the natural. A further comparison of the natural and supernatural, moreover, discloses another reason for the illogicality of the supernatural, and another meaning of the natural as logical.

The supernatural was said to be illogical in that it lacked an intelligible mode of operation. It is not taken, however, to lack a cause. Miracles, demons, angels and gods, are far from being in opposition to the causal principle. On the contrary they illustrate that principle more familiarly than do the propositions of science. It is easier for men, and originally more plausible, to explain events in the light of some one's purposes, even if they are unable to account for the operation of these purposes, than it is for them to explain events in terms of the aimless and mechanical. Anger, jealousy, vindictiveness, reward and punishment, loving-kindness and special favor are thoroughly understandable to them; and what is more to be expected than that they should have read these categories into the world at large and have invented creatures responsible for them. Supernatural accounts are essentially teleological interpretations, and soon pass over into strictly moral interpretations as well, in which the purposes are those of moral agents, demoniac or divine. The reason for events are then given in terms of the good and evil, and not in terms of conditions and logical systems. Thus the faithful are cured because their faith is good, the evil are chastised because of their evil. Bodily structure and material conditions are irrelevant to the course of events, since it is the good as such, or the evil as such, that is taken to be causally efficacious. The goodness of the good and the badness of the evil, however, rests upon the relation of the event or agent to an omnipotent divine will, so that the moral interpretation cannot be divorced from the teleological account which generated it.

In natural accounts, on the other hand, all reason for events are found in their make-up and in their systematic connections with other events. The manner of their generation can be described and stated. Save to a certain degree in the realm of human life, the event is taken to subserve no moral end, nor to be limited by moral or teleological conditions. Hence in natural accounts, control is gotten through knowledge of structure and through the organization of materials appropriate to such structures, techniques irrelevant to moral behavior. The techniques of natural science no more address themselves to moral changes or ends, than aspirants to supernatural intervention address themselves to the conditions or operation of things. Clearly with regard to things inanimate, and even animate, teleological accounts have proven utterly barren, except when applied to human behavior. The way to control has been through knowledge of structure and consequent instrumentation, and not through prayer and magic, because nature is a field in which the reasons for events are structural and mechanical and not moral or purposive. When moral and teleological causes are assigned to events the conditions of which are wholly irrelevant to good and evil, such explanations are illogical in virtue of the fact that the assigned causes have no relation to the events. The distinction between the natural as the logical and the supernatural as the illogical is thus due to the predominantly mechanical type of explanation employed by the former in contrast to the *exclusively* moral and teleological kind assumed by the supernatural.

Although natural accounts always involve structural conditions in their statements, it is not true that every natural event is exhaustively explained by its *material* conditions. A distinction must be made between such events as find their causes in their structure and systematic relations alone, and those which involve in the account of their causes a reference to teleology as well as to structure. When this distinction is made, and it *is* made in colloquial usage, the words 'nature' and 'natural' become equivocal. They are at once used to designate the realm of all that is capable of logical explanation, and also to differentiate the realm of products explanation of which is irrelevant to purposes from

the realm of products explanation of which does refer to purpose, that is, the realm of art. The difference between nature and art, when these are used as antithetical terms, is a difference in the way things are produced. In both realms the products are logically explicable, and their behavior homogeneous. A table produced by art is characterized by the same physical laws as the stone produced by nature; and it is in virtue of this identity in order, that the products of art are included in the general field of nature. The distinction between art, and nature in the limited sense, is that between realms composed of things differently produced. This distinction can best be made in terms of the productive orders of art and nature.

Art involves inquiry; and an exhibition of the order of art must consider the order of inquiry. Inquiry begins with a product and proceeds to the origin, structure or condition of it, so that its order is from product to origin. The object of inquiry, however, is to attain knowledge of the order of nature, and this is a passage from origin to product. The order of ideas in inquiry is, thus, the reversal of the order of things in nature, but as stated in knowledge becomes identical with it. Inquiry does not end with an actualization of nature's order, in that nothing is produced, no product is generated; but it does end with a representation of nature's order, with a statement of what it is. It ends with the order of nature in *idea*, not in actuality.

The order of art is not identical with either of these. Like the order of inquiry, art begins with a product and goes to its origin, except that it begins with a virtual product and goes to a virtual origin. The artisan does not, as in inquiry, examine real products and real origins; yet his going from the virtual product to its virtual origin is guided by what knowledge he has of actual products and their actual origins, or things of like kind. The order of art, in other words, is mediated by the order of inquiry. Then it reverses the procedure and transforms the virtual origin into a real origin, and thence produces a real product. Here, again, it differs from inquiry, whose consummation is the representation of the order of nature. Art does not content itself with merely representing nature's order, but ends

by affecting it. The things the artisan uses to generate the product are then called 'tools' or 'instruments,' without which his knowledge would be ineffectual. Art thus requires both planning and execution. The first is controlled by knowledge, and the second by instrumentation. Art is a process which begins by rehearsing the order of inquiry, and ends by effecting, not representing, the order of nature. If it be asked: what is the reason for the product of art, the answer must doubtless be in terms of its use as well as its origin. Hence the products of art are differentiated from the products of nature in that they are produced for an end. It is not merely that they are utilized for an end, since the products of nature are also so utilized. It is that they are *generated* for an end. The manner of their generation can be described and the knowledge transmitted to others. Consequently, the products of art are wholly in accord with natural laws. They are logically bound, as the events in nature are, with a system of things. It is in virtue of this that knowledge of nature is indispensible in art, and that the order of its execution is the same as nature's order.

Nature, in this limited sense of contrast with art, therefore, signifies that realm of things whose order of production is from origin to products simply, and whose production is explicable in wholly mechanical terms, involving no reference to purpose. The distinction between it and art is thus of the same kind, in part at least, as that between nature and the supernatural. Whereas the latter term involved the irrational as well as the teleological, art signifies that which is teleologically but rationally produced, the teleology itself being explicable according to the laws of psychology, and the execution in accordance with the laws of nature in the limited sense. As such, art too is included in the wider sense of the word 'natural,' which, consequently, must be taken to signify the logical, although its narrower meaning signifies the mechanical merely.

The definition of nature as the logical, however just, is wholly inadequate, for it denotes a skeleton from which the flesh and movement have fallen away. The world, the cosmos, the universe and existence, are synonymous with nature, and these are

taken to include the heavens, the earth, the sea and all that in them is. The cosmos, beyond its logical structure, is the theater of comings and goings, of passage and alteration. It is the collection of things which *act*, which come into being and pass away, which produce and are produced. It rejects such things as supernatural entities, fictional and mythological creatures, the objects of dreams and some false propositions, and even, sometimes, the divine. When the things of existence are distinguished from dreams and inventions of the imagination, productivity is what makes the difference. "Mental fires is what won't burn real sticks; mental water is what won't necessarily (though of course it may) put out even a mental fire. Mental knives may be sharp, but they won't cut real wood. Mental triangles are pointed, but their points won't wound. With 'real' objects, on the contrary, consequences always accrue. . . ." The productivity of nature is not incompatible with its logical order. Although logic and effectiveness are not the same thing, since effectiveness involves alteration, every case of effectiveness is a case of logical bondage. What else is meant in saying that the objects of dreams are ineffective, but that they are not logically bound to the things with which, so to speak, they claim to be? The consequences that ought to accrue do not accrue. Therefore the events in the dream are said not to exist. Their existence and effectiveness and logical tie are all one. In the larger sense, then, nature is the cosmos, the world, the universe. It is the collection of effective individuals, of individuals that are parts of systems, of movements from origin to product, of comings and goings of histories. It is also the collection of systems of events logically ordered, and of orders of effectiveness.

Although nature is the totality of effective things, it is not itself effective, nor does it affect anything else. Effectiveness characterizes the parts of nature only, not nature as a collection, for if nature were effective or effected, one of three alternatives would be the case. Either nature would be both agent and patient, or agent only, or patient only. In any of the alternatives, an effective and therefore logical tie would obtain between the alleged nature and not-nature. Both then would be parts

of a logically and actively related system; and *both* therefore would fall into the field denoted by the word 'nature.' Hence, if there be anything outside of nature, it neither affects nor is affected by nature. It can have no relation to it, nor could there be any knowledge of it. To posit the existence of such an entity would be meaningless, for what sense would the word 'existence' as applied to it have? Nature, then, is unconditioned, and there is none other beside it. Since it is neither agent nor patient, it cannot be said to be created or creator. If it were created, its creator would have affected it; and if it were creator, the product would be affected by it. Movements from origin to product thus characterize the *parts* of nature, and not nature itself, which neither comes into being nor passes away, neither binds nor is bound, neither acts nor is acted upon. These things may be said of its members, but it itself is wholly unconditioned, without limitation, and self-existent.

The entities and systems of which nature is the collection, are effective entities and systems, logically explainable. These characteristics are precisely what is meant by the term 'natural,' effectiveness and logical coherence not being antithetical or mutually irrelevant, but distinguishable aspects of one principle which embraces them both, the *causal principle*. This it is therefore which should be controlling for a theory of nature. Such a theory must begin with an examination of causality[3] and this examination leads back again to the distinction between nature as synonymous with existence, and nature as differentiated from art, in virtue of the difference between mechanical and teleological causation. In the relation between these lies the ground for the understanding of life and art as well as nature, and of their comprehension within it.

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[3] See: Lapan, *The Causal Situation*, The Journal of Philosophy, vol. XXXIV, no. 7, April 1, 1937.

A Re-examination of Some Arguments for Realism

BY

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FOR the purposes of this paper, *realism* is defined as the belief that in visual perception there is a direct perception of material bodies existing in space external to the perceiver's body. Most contemporary positivists and analytical philosophers are realists in this sense. Included in this classification would be all those who argue from the character of seen relations of bodies to the uniformity view of causation; those who oppose public (extended or in space) to private experience; those who believe that one can point to facts; those who believe in the *real* existence of objects like extended solid tables and chairs; and those who distinguish in terms of verifiability between judgments of color and judgments of value.

In attempting to discover why anyone would hold such a doctrine in spite of the fact that "the rejection of the view that perception gives direct knowledge of external objects is as certain as anything in science can hope to be,"[1] I examined many subtle arguments with which realists and analytical philosophers defended their right to be naive. In all the material examined, I found only two relevant arguments, both patently fallacious, and a kind of historical confusion or blindness. In what follows, I shall exhibit the fallacies and the confusion, not to refute realism

[1] B. Russell: *Analysis of Matter*, p. 197.

in emulation of Professor Stace—I had always supposed that the Democritian refutation of direct realism, at least, was final—but in the hope that those who are concerned with the truth of the matter will abandon dialectical refutations of one another and turn their attention to a re-examination of the facts involved.

In his *Refutation of Idealism*, Professor Moore treated the *esse est percipi* as the basic premise of all idealisms. And this emphasis on the function of a premise which this notion filled in certain types of idealistic constructions seems to have blinded many subsequent investigators to the fact that the *esse est percipi* is the *conclusion* of an elaborate structure of fact, theory and argument. I can only account of the wide use and acceptance of certain arguments on the supposition that this failure to consider the development of which the *esse est percipi* is the conclusion had deadened the critical faculties of realist philosophers. But let me proceed to show this more concretely.

"In 1879 Fechner describes the state of affairs as to the doctrine that the immediate objects of sense are only modifications of the mind in these words: 'This is the opinion of all thinking men about me. In whatever else they may disagree, on this point the philosophers and the scientists, the materialists and the idealists, the Darwinians and the anti-Darwinians, the orthodox and the rationalists are unanimous. It is not one stone in the fabric of the contemporary world view, it is the foundation stone.' "[2] The sense of this passage can be expressed more specifically for our purposes in the following manner. All intelligent men agree that the immediate objects of sense are modifications of sentience and are not parts of nor the surfaces of material bodies; but from this point on, we find disagreement. The positivist believes that apart from modifications of sentience, there is nothing else; the materialist believes that the modifications of sentience are caused by the movements of unperceived external material bodies; the absolute idealist believes that something does exist external to individual sentience but that what so exists is to be understood as a modification of absolute sen-

[2] S. Z. Hasan: *Realism*. Cambridge, At the University Press, 1928, p. 46.

tience; the neo-Kantian believes that what exists external to individual sentience is in principle unknowable.

It can immediately be seen that the *esse est percipi* so far as it applies to sensible things is not a doctrine unique to idealism. It may be the case, however, that certain idealists have attempted to argue dialectically from the *esse est percipi* so far as it applies to sensible things to the *esse est percipi* taken with complete generality. Such an attempt might be dialectically refuted and if Professor Moore has done anything at all in the *Refutation of Idealism* and *The Nature and Status of Objects of Perception* he has done just that. But, apparently, there was a time when he thought he had done more, for he says, "*Esse* is *percipi* in the sense I have found for it *may* indeed be true; I cannot refute it; but if this sense were clearly apprehended, no one, I think, would believe that it was true."[3] (Moore here is not talking of true in general but true for sensible objects.) The sense which he finds for the *esse est percipi* is merely that it is synthetic and not analytic, i.e., not a definition. And Professor Moore argues that once the synthetic nature of the proposition is granted, the only reason for believing it is true lies in confusing the act of sensing with the content sensed.

I am not concerned with the number of absolute idealists or phenomenalists who have been guilty of this confusion. But surely Berkeley was not so guilty, nor Descartes, nor Malebranche, nor Leibniz, nor Locke, nor Kant, nor Bradley, nor Whitehead, nor all the thinkers involved in Fechner's generalization. For the sake of the argument, let us grant again that Professor Moore has refuted something which can be called absolute idealism. It still remains the case that he has not even considered the reasons for believing, the reasons stated over and over again throughout the history of science and philosophy for believing, that *esse* is *percipi* so far as it applies to sensible things. Professor Moore may not himself believe that we directly perceive the surfaces of material bodies[4] or that sensible

[3] G. E. Moore: "Refutation of Idealism." *Philosophical Studies*, p. 12.
[4] Op. Cit., p. 195–6.

objects do exist unperceived, yet, I feel, he is more than an innocent cause of the realist debacle. To conclude that, once it is held that the *esse est percipi* is synthetic and once the sensing is distinguished from the sensed, no one would believe that *esse est percipi* is true, is to prepare the way for a Roman holiday for all the Epigones who know what they know and see what they see. It is to encourage and condone neglect of the history of science and philosophy so that Professor Lovejoy is forced to remark that "While misconceptions of the dualist's arguments thus had no small part in causing the revolt, the new movement [neo-realism] perhaps owed not less to sheer lapses of memory on the part of philosophers."[5]

I proceed now to discuss the two fallacies. They are the only arguments I have ever been able to discover in contemporary philosophic writings against the acceptance of the *esse est percipi* so far as it applies to sensible things. Only two brief and, as I have remarked above, patently fallacious arguments. I can only explain their wide acceptance among realists and analytical philosophers by referring to the attitude sponsored by Professor Moore. After all, if no one would believe the *esse est percipi* in the only sense in which it would be significant (as synthetic), then no arguments are really required. Anything will do and we can pass on to more important questions.

I shall take examples of the first fallacious argument from Professor H. H. Price's book on perception, although its duplicate could be found in the writings of almost any realist. I choose these examples because Professor Price is one of the most careful and rigorous of recent writers on perception. But first let me state briefly what the argument is so that the attention of the reader will be directed to the relevant sentences in the long quotations which are to follow. Professor Price holds that the view that what we see is caused by and is not a part of the external world is self-contradictory since it is based upon regarding some of those things we see as part of the external world.

[5] A. O. Lovejoy: *The Revolt Against Dualism.* Open Court, 1930, p. 44.

"Thus if it be said that when a man sees something, e.g. a tomato, light rays emanating from the object impinge upon his retina and this stimulates the optic nerve, which in turn causes a change in the optic centers of his brain, which causes a change in his mind: there are two comments to be made. 1. No doubt this is in fact a perfectly true account, but what are our grounds for believing it? Obviously they are derived from observation, and mainly if not entirely from visual observation. Thus the Physiologist has not explained in the least how visual perception justifies a man in holding a certain belief about a tomato, e.g. that it is spherical. All he has done is to put forward certain other beliefs concerning a retina and a brain. These other beliefs have themselves to be justified in exactly the same way as the first belief. . . . Since the premises of Physiology are among the propositions into whose validity we are inquiring, it is hardly likely that its conclusions will assist us."[6]

Professor Price is here saying that since it is a premise of physiology that visual perception is a direct perception of entities like nerves and brains, the causal account of perception cannot be taken seriously as an account of the relation of what we see to the external events which are their causes; and that the science of physiology can supply us with no relevant evidence for the truth of the *esse est percipi* so far as it applies to the immediate objects of visual perception. The next passage will bring out these points more clearly.

"The Causal version of the Argument from Illusion is not nearly so easy to defend. It is constantly talking of physical media, of mirrors, lenses and prisms, of drugs and physical diseases, and especially of sense-organs, nerves and brains. Thus it presupposes a vast amount of detailed knowledge concerning a variety of material objects. And this knowledge is certainly not *a priori*. It is got from observation: it presupposes both sensing and what we have called perceptual consciousness. . . . Thus whatever the argument proves, it certainly cannot have the slightest tendency to prove that there are not material bodies to which sense-data belong . . . otherwise it would be contradicting its own premises."[7]

[6] H. H. Price: *Perception*. London, Methuen & Co., 1932, p. 2.
[7] Op. Cit., p. 36.

Both of these passages occur before Professor Price formally begins his discussion of the causal theory; but in the chapter specifically concerned with this theory he says:

"... the official foundation of Natural Science ... is the *Causal Theory*. Unfortunately some writers mean by this simply the ordinary physico-physiological account of the way in which sense-data are as a matter of fact generated, by the action of other material things upon the sense organs and nervous systems of sentient beings. That, however, is merely a part of Natural Science and has no claim to be the foundation of it. Like all other parts of natural science it is based upon observation, in this case observation of lenses, retinae, ears, brains and the like; but it is not a theory of what observation itself is, which is the question which concerns us as philosophers. The only sort of Causal Theory which is of philosophical importance is ... the theory that belonging to M is the same as being caused by M, and that perceptual consciousness is an inference from effect to cause.... This, then, is what we in this book shall always mean by the Causal Theory."[8]

Here is an example of the historical confusion and its consequences in philosophic analysis. "The ordinary physico-physiological account of the way in which sense-data are as a matter of fact generated" may or may not be the official foundation of natural science; it may not be the account which Professor Price intends to analyze any further than he has done in the first two passages quoted above; and it certainly is not the account which Professor Price intends to analyze under the name Causal Theory; but it is *this account* which forms the scientific foundation of the conclusion that *esse* is *percipi* so far as it applies to sensible things; and the question of the compatibility of this account with any one of the variety of realisms seems to me to be of prime philosophic importance.

Turning, now, to the first passage, Professor Price believes that a theory of perception, whether advanced by a physiologist or anyone else, must explain "how observation justifies a man in holding a certain belief about a tomato, e.g. that it is spherical," and that the physiologist's account of perception rests upon

[8] Op. Cit., p. 67.

similar beliefs about the brain which require justification, e.g. that it is gray and oval-shaped. I think, however, that if physiology justifies a man in holding any belief about tomatoes it is certainly that they are not spherical or, rather, that the perceived shape is a function of the perceiving organism and that it is not an independently existing quality which is part of or belongs to the external physical object. But such a view, Professor Price suggests in this passage and states definitely in the second passage, contradicts the premises of physiology which, according to Professor Price, are that brains, which are essentially like tomatoes, have shape and color independently of the observer.

I am not denying that physiologists speak of mirrors, brains, nerves and the like; but it is ridiculous to suppose that the truth of all propositions in physiology is based on the theory of the direct visual perception of external objects. Such a state of affairs would indeed convict most physiologists of being rank fools. But by what line of argument is it established that the visual perception of the brain is a direct perception of the colored surface and shape of a material body? Just as in the case of the tomato, the physiologist could and does hold that his visual perception of the brain is a modification of his own organism caused by the action of physical objects, including his own body, in the environment. To this Professor Price could reply—and this answer is assumed by most realists to be devastating—"If all our sensible perceptions are modifications of our own sentience, how can we know anything else such as the cause of such modifications?" This question is usually asked with such an air of triumph that it seems a shame to prick the bubble. I have answered it in another place, not through any original discovery or argument, but by repeating what is essentially the answer of Locke, Leibniz, Berkeley and Whitehead. Quite recently Russell has given a similar answer.[9] The answer consists simply in pointing out that all our direct experience is not exhausted in sense experience. There are other types of direct

[9] B. Russell: "The Limits of Empiricism" *Proceedings of the Aristotelian Society*, N. S., Vol. 36, 1935-36, pp. 131-150.

experience and from them we get our ideas of causation, substance, external existence and the like. Thus Leibniz, in commenting on certain views advanced by Locke, says: "Now the soul comprises being, substance, unity, identity, cause, perception, reason, and many other notions which the senses cannot give. This view sufficiently agrees with your author of the Essay, who seeks the source of a good part of ideas in the spirit's reflection upon its own nature." And "Thus I think that the idea of existence comes from reflection. That of power also and of unity come from the same source."[10]

There is no room to say more about this matter here. The arguments are a matter of historical record and can be referred to by anyone who, differing from some contemporary analytical philosophers, has interest in anything so plebian as the study of what the great philosophers have said. Perhaps, however, it should be recalled in this place that for both Locke and Leibniz *reflection* was a part of direct experience and by no means identical with the modern sense of *a priori* knowledge. Further, I am not saying that everything said historically in support of this thesis is true. But I do think the position is of sufficient importance to merit some consideration from realists and writers on perception. After all, in three volumes (all of which appeared earlier than Professor Price's *Perception* and to which he has not a single reference) Professor Whitehead has worked out a detailed view of the direct non-sensuous perception of the external world; and the doctrine he advances is but a development of one of the main currents of European philosophic speculation from Plato's day to our own. From Descartes through Bergson to Russell and Eddington, philosophers and scientists have protested against the tendency of "common-sense" and hard-headed realists to interpret the external world in terms of the extended character of a visual sense-datum. Hence, it must be regarded as incredibly naive, if not illogical, to assume without any argument, as does Professor Price and others, that, if visual

[10] G. W. Leibniz: *New Essays Concerning Human Understanding.* New York, The Macmillan Co., 1896, pp. 111, 130.

and tactile sensation did not give us knowledge of the external world, there could be no such knowledge.

I turn now to the consideration of the so-called "selective view" of sensation; the following is Professor Moore's statement of the position:

"The argument is one which asserts that there is abundant empirical evidence in favor of the view that the existence of sensibles which we experience at any time, always depends on the condition of our nervous system: so that, even where it also depends upon external physical conditions, we can safely say that sensibles, which we should have experienced, if only our nervous system had been in a different condition, certainly do not exist when it is not in that condition. And the fallacy of this argument seems to me to lie in the fact that it does not distinguish between the existence of sensibles *which* we experience and *the fact that we experience them*. What there is evidence for is that *our experience* of sensibles always depends upon the condition of our nervous system; that, according as the condition of the nervous system changes, different sensibles are *experienced*, even where other conditions are the same. But obviously the fact that our experience of a given sensible depends upon the condition of our nervous system does not directly show that the existence of *the sensible experienced* always also so depends. The fact that I am now experiencing this black mark is certainly a different fact from the fact that this black mark exists. And hence the evidence which does tend to show that the former fact would not have existed if my nervous system had been in a different condition, has no tendency to show that the latter would not have done so either."[11]

This is a long passage containing as it does the briefs of both contesting parties and I will restate its crucial sentence: "And the fallacy of this argument seems to me to lie in the fact that it does not distinguish between the existence of sensibles and the fact that we experience them." The argument supposedly fallacious is that drawn from psychology and physiology (based upon "abundant empirical evidence") which holds that the nervous system is a partial cause of the existence of what is experienced. Hence the first answer to Professor Moore is that

[11] G. E. Moore: "The Status of Sense-data." *Philosophical Studies*, p. 183–4.

neither psychology nor physiology is interested in facts like the "existence of sensibles" or the "existence of this black mark" if such facts are supposedly distinct from the experience of sensibles or the experience of black marks. Professor Moore might reply that since psychologists and physiologists are not concerned to make the distinction, then they cannot be referred to in discussing the existence of such facts as the unperceived existence of black marks. Suppose this last be granted; we may then ask to whom we must refer for such a discussion. There is surely no science or branch of knowledge, unless it be some recent enterprise of analytical philosophers of which I am unaware, which concerns itself with the unperceived existence of sensibles. Further, where shall we get, not "abundant empirical evidence", but any empirical evidence for the validity of the distinction and the existence of such facts. Professor Moore's demonstration that the *esse est percipi* must be synthetic to be significant is not evidence. For that demonstration establishes only that *something may be* without being perceived. For all the demonstration proves, as Professor Moore is careful to say over and over again, it might still be the case that there has never been and never will be an actual case of a sensible existing unperceived, in which case it would scarcely be intelligent to ask a physiologist to distinguish *such facts* from other facts. There remains the final and, I think, conclusive consideration that no satisfactory meaning can be assigned to the term existence when applied to unperceived sensible things.

I shall close with a few remarks on the so-called "balance of proof." Aside from the two arguments I have examined, the only other argument I know for the thesis that sensibles exist unperceived is the appeal to common sense. It is thought that once the bare logical possibility of the realistic thesis is established, the overwhelming conviction of mankind will operate to sustain it. Quite recently we find Professor Montague saying, "And as Dr. G. E. Moore has remarked, if only the idealist can be made to entertain even as a bare possibility the hypothesis that the objects of which we are aware may nevertheless be independent of that awareness, half the battle for realism is

won."[12] And Professor Price says, "And this suggests that our conviction of the existence and squareness of the table may not after all require external justification at all. . .It is certain that no one except a few philosophers has ever thought such support was needed or offered to supply it."[13] Here are two excellent examples of the forgetfulness of which Professor Lovejoy spoke, though I suppose that it is legitimate to speak of the long line of scientists and philosophers who doubted the independent existence of sensible objects as a "few" if they are to be compared with the immense multitudes who never thought at all. On this point I might modify a well known saying, namely, "the philosophy that lives by appealing to common sense, will perish by it." Common sense is scarcely concerned to protect individual philosophic differences. Realists who, in lieu of arguments drawn from their own resources, invoke the aid of common sense in their battle against idealisms, should ponder the fate of Agathocles, Tyrant of Sicily.

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[12] W. P. Montague: "The Story of American Realism." *Philosophy*, Vol. XII, No. 46, April, 1937, p. 145.
[13] Op. Cit., p. 68.

Science and the Philosophy of Science

BY

A. C. BENJAMIN

THERE are some indications that the philosophy of science is reaching the age of discretion. Now, as I understand it, the age of discretion is characterized by self-examination. Youth is a period of blundering enthusiasm. But maturity demands the sobering influence of principles, perspectives and techniques. The adult must put away childish things. This does not demand the elimination of spontaneity and imagination, but it does require their chastening according to the principles of propriety. It seems time to ask ourselves whether the philosophy of science ought to be allowed henceforth just to grow, like Topsy, or whether a serious attempt ought not to be made to determine its nature and task.

As a step in the direction of defining more precisely the scope and method of the philosophy of science I should like to propose that the essence of this problem seems to lie in the nature of the relation which the philosophy of science bears to science. This appears to be an elementary problem. The philosophy of science is about science, and that seems to be the end of the matter. But the precise way in which the philosophy of science is about science is not at all clear. It is not at all evident that there is any sense in talking about a discipline which studies a discipline. It is certainly not obvious what the method of such a discipline would be. And it is far from clear just what in science the

philosopher studies when he studies science. Does he study assumptions, methods, facts, symbols, operations, or scientists?

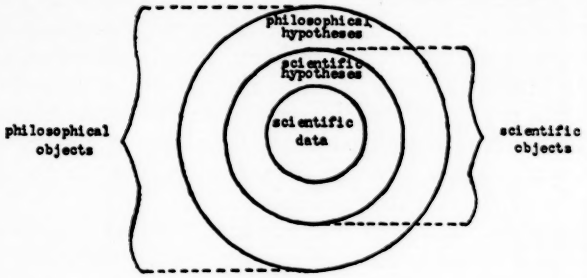
The hypothesis which I wish to suggest as a solution to this problem of the relation between science and the philosophy of science is not novel, nor is it highly complex and involved. It is, in fact, the hypothesis which would naturally occur to one. It is both naïve and, in a certain sense, perfectly obvious. Yet I venture to offer it for reconsideration since its exact nature has seldom been made clear and its explanatory possibilities have seldom been explored. On the basis of this interpretation I should like to suggest certain objections which might be raised to the legitimacy of such a discipline, and to show the direction which an answer to these objections would take.

I

In its simplest form the thesis states that the relation between the philosophy of science and science is in general outlines the same as that between science and nature. Science studies nature and the philosophy of science studies science studying nature. This means that the data and problems of the philosophy of science can be found in science itself, broadly understood, in the same way that the data and problems of science can be found in nature. Furthermore, if the task of science is that of describing nature and formulating the most reasonable hypotheses in terms of which nature may be explained it follows that the task of the philosophy of science is that of describing science and formulating the most reasonable hypotheses in terms of which science may be explained. In both cases there is a problem, and there are data and hypotheses by means of which a solution may be achieved; in both cases the solution is not final but represents the most probable conclusion on the basis of the data and techniques available. In fact the difference between the two studies seems to be mainly terminological. If the task of science is to explain scientific data by means of scientific hypotheses then the task of the philosophy of science is to explain philosophical data by means of philosophical hypotheses but *the philosophical*

data are precisely the scientific hypotheses in their relationship with the scientific data.

The nature of this interrelationship may be clarified by a diagram. The small circle represents the realm of scientific data, and the ring which surrounds it represents the realm of scientific hypotheses. The total area covered by the larger of these two circles represents the realm of scientific objects, data and hypotheses, in the relationship described by saying that the hypotheses explain the data. The largest ring represents the realm of philosophical hypotheses; these bear the same relation to the realm of scientific objects that the scientific hypotheses bear to the scientific data. This large circle designates the total area



of philosophical objects, data and hypotheses, which includes, of course, all scientific objects.

The purpose of this diagram is twofold. It aims in the first place to indicate the continuity of philosophical hypotheses, scientific hypotheses and scientific data. These are arrangeable on a radius according to "distance" from the center. Philosophical hypotheses are more "distant" from scientific data, and if "distance" is identified with "incomfirmability" then scientific hypotheses are more completely confirmable than philosophical hypotheses. But the continuity of the dimension suggests that the *method* of philosophical confirmation is essentially the same as that of scientific confirmation, though the *degree* may differ. The diagram aims in the second place to point out that the relation

of the larger ring to the enclosed ring is always that of explanation. Hence it becomes possible to say that philosophy explains science in the same way that any scientific hypothesis explains its data. The fact that the diagram is limited to three circles should not be taken as essential. There is some reason to believe that the realm of scientific data should itself be represented as a ring which surrounds an area capable of being designated as the realm of common sense objects, e.g., the realm of having, doing, enjoying, and using, and that the relation of scientific data to this realm of common sense objects is essentially the same as the relation of scientific hypotheses to scientific data. This would argue that the facts of science explain common sense in the same way that scientific hypotheses explain scientific data. Similarly there may be a realm of meta-philosophical objects which are required to explain the relation between science and the philosophy of science; in fact it will be argued in the concluding paragraph of this article that the thesis here developed, i.e., that philosophical hypotheses explain science, is itself a meta-philosophical hypothesis and as such must lie beyond the realm of philosophical objects.

It should be made clear that this conception of the philosophy of science does not constitute an unnecessary limitation of the scope of its subject matter unless science itself is too narrowly conceived. But the history of thought seems to justify two important conclusions. (*a*) It is impossible to assert with any finality what the limits of the scientific method are, and, consequently, what methodological features a discipline must exhibit in order to be a science. New techniques are continually emerging, and old techniques are progressively being extended beyond their original scope. (*b*) It is impossible to assert with any finality what the range of data for any given science is, and, consequently, what features of subject matter distinguish one science from another. The progressive discovery of border-line data argues more and more for the continuity of nature. Hence, all that one can say in effect is that the realm of scientific data is the total realm of entities which can be known by certain techniques whose possibilities have not been completely explored,

and that the range of scientific techniques is that totality of methods which are adapted to the exploration of a realm of objects whose boundaries have not yet been found. Practically one can avoid this difficulty by using the denotative method and describing the realm of scientific objects as that totality of entities which are or may be made known by any science, formal or empirical, descriptive or normative, behavioristic or introspective, statistical or individual, natural or social. Anything short of this seems to constitute an unjust limitation of scientific subject matter.

But if the term "science" is given this liberal interpretation there is no reason to be disturbed over the dearth of philosophical objects. Philosophical data will include every fact of science and every explanation of such fact provided they are taken in the relationship of explained-to-explanation. And philosophical hypotheses will include every attempt to explain scientific cognition. Hence the relevance of philosophy is determined by its capacity to explain science, broadly conceived, and the verification of any philosophical hypothesis must be found in science. Certainly this provides adequately for logic, epistemology and the general theory of signs. All of these disciplines may be considered as attempts to formulate hypotheses which will explain science in the same way that scientific hypotheses explain scientific data. But such a conception of philosophy also includes what is commonly called "metaphysics"—at least in the sense of empirical metaphysics or scientific cosmology. Whether one considers metaphysics in its analytic phase, as the determination of the meanings of the basic concepts of the sciences, or in its synthetic phase, as the integration and unification of the special sciences,—in either of these cases metaphysics will have a direct relevance to and verifiability in terms of science. Similarly, all axiological studies which attempt to examine and criticize the value concepts of the normative sciences, and all speculative attempts to evaluate values and to point to the ultimate and final value—all such studies may be considered as efforts directed upon the problem of the integration of the sciences, and any attempt to solve such problems must be in terms of philosophical

hypotheses verifiable by an examination of the sciences in their broader contexts. In fact if anything to which one may react in any way is a legitimate object for scientific study, and if the scientific explanation of such an object is a legitimate object for philosophical study, then the field of the philosophy of science is unlimited. The philosophy of science becomes identical with philosophy in general.

II

I have thus far attempted to show that the relation between the philosophy of science and science is essentially the same as that between any hypothesis and the corresponding data within science itself. This argues for a continuity of subject matter and method as one passes from science to the philosophy of science. But it also argues that any differences which characterize the two disciplines will also be of the kind which distinguish scientific hypotheses from scientific data. Hence I propose now to examine some of the objections which may be directed against the possibility of the philosophy of science, and to show that exactly the same objections may be directed against the possibility of scientific hypotheses. In fact the historical controversies as to the nature and status of scientific hypotheses have their precise modern parallels in disputes as to the nature and status of philosophical hypotheses. Let me point out some of these objections.

(1) *The philosophy of science is too vague and speculative to warrant serious consideration.* There are really two objections here: (*a*) The philosophy of science is vague and speculative, and (*b*) Vague and speculative ideas do not warrant serious consideration. In each of these there is a scientific analogy.

(*a*) While philosophical hypotheses may be vague and speculative it is also true that scientific hypotheses are vague and speculative *by comparison with the data in terms of which they are verified*, and that there is no satisfactory way of distinguishing scientific hypotheses from scientific data. Much of a hypothetical character is contained in the simplest fact, and every hypothesis differs from every other in the amount of factual

material which it "contains", i.e. in the degree of its verification. Probably ultimate verification must be made in terms of denotative gestures; certainly the most obvious facts are those which can be pointed to and described by simple assertions such as "this is white", "this is a triangle", "this is a pointer indicating 5 on a scale", etc. Reichenbach calls this the "peremptory character of immediate things" and he assigns to propositions asserting their existence the highest initial weight.[1] But he makes clear that "there is no statement concerning physical objects which is absolutely verifiable."[2] This indicates that there is something of a hypothetical character even in the most immediate experience, and the distinction between data and hypotheses becomes one of the degree of probability (or weight, in Reichenbach's terminology) which one is able to assign to the propositions asserting their existence; propositions having a high degree of probability assert data, propositions having a low degree of probability relative to these assert hypotheses.

It seems that similar considerations may be applied to the verifiability of philosophical hypotheses. While the weight of a philosophical hypothesis may be great relative to the philosophical data which it explains, these data are themselves hypothetical with reference to judgments about immediate things, hence the final weight of a philosophical hypothesis will not be great relative to judgments of perception. This is indicated in the diagram by the "distance" which separates philosophical hypotheses from scientific data. Thus as one leaves the center of the inner circle and passes to the outer circle the data may be said to be more obscurely given and to require verification by more and more elaborate operational routes. Philosophical objects, as illustrated by Plato's Ideas, are not seen by the ordinary man except on rare occasions and as a result of prolonged training. Dispute about such objects seems endless simply because of the impossibility of using the denotative method for direct verification and because of the complexity of the indirect verificatory routes which must be employed as substitutes. Problems of

[1] *Experience and Prediction* (University of Chicago, 1938), p. 275, p. 280.
[2] *Ibid.*, p. 87.

subjectivism-objectivism, monism-pluralism, materialism-idealism, theism-atheism, and the rest are not meaningless problems nor different in kind from the problem of quanta-continuity in physics.[3] Philosophical hypotheses differ from scientific hypotheses only in the complexity of the verificatory route and in the elusiveness of the data by which they are verified.

(*b*) The decision as to the boundary between science and the philosophy of science is arbitrary in the sense that after the investigator has drawn the circle indicating the extent of scientific data, he is free to choose whether or not to pay any attention to the entities outside of the specified area. Carnap has shown[4] that one's criterion of testability is a matter of free choice; one can accept a generous standard in which case all of science, and perhaps much of philosophy, will be accepted as meaningful; or one may choose a more limited criterion in which case many statements admitted by most people to be sensible will be characterized as nonsense. Now it has been true in the history of science that investigators have been divided rather sharply into two groups: the positivists who are afflicted with hypotheseophobia and insist that the task of science is the mere description of the data, and the anti-positivists who are more generously inclined and argue that description must be supplemented by explanation and hence that hypotheses have a distinct rôle to play in science. Apparently some scientists can get along without hypotheses, or at least are compelled to place less reliance upon them than others—and that seems to be the end of the matter. There is no question of truth or error involved.

The situation is the same with regard to the boundaries between science and the philosophy of science. It is commonly recognized that there are scientists who are contemptuous of the philosophy of science and scientists who value it. The former may be anti-metaphysical, scorning all investigations into "ultimates", or merely anti-logical, insisting that science proceeds most rapidly by being let alone; the latter feel that investigations

[3] See, as an example, the inductive proof of realism. Donald Williams, in *Monist* vol. 44.

[4] "Testability and Meaning," *Philosophy of Science*, vols. III and IV.

into techniques, presuppositions, and integrations have important contributions to make to science. The former are charged with being simpleminded, and the latter with being muddleheaded. But in neither case is praise or blame called for; the question is merely one of making a decision and then accepting the consequences.

While these considerations do not answer the objection they do show that there is something of an arbitrary character in it. The scientist may still claim that the philosophy of science is too vague and speculative, and that it is unimportant. But if he admits that important discoveries sometimes arise through talking about scientific hypotheses which are vague and speculative relative to scientific data, he must admit at least the possibility that philosophical hypotheses are fruitful in the same way. Or at any rate he is under obligation to show why he chooses to draw the line at one point rather than at another.

(2) *Philosophical hypotheses are mere linguistic conveniences, logical constructions, and heuristic devices to be ultimately abandoned; hence philosophy has no explanatory value.* No scientist who is familiar with the history of recent science would employ such an argument as this. Science is not merely the science of Mach, Kirchhoff and Pearson, but of Meyerson, Planck and Bavink. For Pearson "atom and molecule are intellectual conceptions by the aid of which physicists classify phenomena and formulate the relationships of their succession."[5] "To no concept, however invaluable it may be as a means of describing the routine of perceptions, ought phenomenal existence to be ascribed until its perceptual equivalent has been actually disclosed."[6] But there are also realists in science who insist that hypotheses are designations of natural objects somewhat more obscurely given than the data but nevertheless genuinely real and required in order to explain. "Atoms are just as real things as cannon balls or grains of sand, as waves on water or mountains."[7] Although we can see cells through a microscope "in principle

[5] *Grammar of Science* (Macmillan, 1911), p. 95.
[6] *Ibid.*, pp. 277.
[7] B. Bavink, *The Natural Sciences* (Century, 1932), p. 29.

Rutherford is doing no more than this when he interposes between the flying atom and the eye a somewhat more complicated apparatus consisting of an ionisation chamber together with a galvanometer; only in this case the way is somewhat longer. All investigation of nature depends upon an enlargement of our senses of this description."[8]

Precisely the same controversy has arisen in recent years as to the status of philosophical entities. It has centered on the question as to the nature of logical objects. On the one hand are certain of the logical positivists[9] who insist that logical objects are only linguistic objects, and that all propositions of logic are tautologies which are analytically true by virtue of certain conventions of language. They assert nothing, and there are therefore no entities called "logical objects" which are their designata. On the other hand, there are some[10] who have been championing a logical realism which insists that logic is about the world in essentially the same sense that any specific scientific proposition is about the world. Logic may be about language, but language is about the world, and the form of language—if language is to be considered as a genuine device of communication, and not merely as the rules for playing a game—must be determined by the form of fact. This is simply the Pearson-Bavink controversy with the illustrations of scientific hypotheses replaced by illustrations of logical entities such as propositions, classes and implications. Logical objects may be merely linguistic conveniences, or they may be as real as cannon balls or grains of sand.

Here, again, of course, the objection is not destroyed; but its force is considerably weakened. Philosophical entities are not necessarily conventions. Furthermore, even if they are one cannot object to them if he admits that scientific hypotheses are mere conventions and yet have explanatory value. If any

[8] *Ibid.*, pp. 28–9.

[9] Wittgenstein, *Tractatus Logico-Philosophicus*, (New York, 1922), prop. 4.462; Carnap, *The Unity of Science* (London, 1934), pp. 33–4.

[10] See my article "Outlines of an Empirical Logic," *Philosophy of Science*, vol. 3. Recent discussions with D. C. Williams, E. J. Nelson and J. H. Woodger have convinced me that logical realism is a view which has an increasing number of adherents.

convention is of use, then a philosophical convention may be of use. And if philosophical hypotheses may not be conventions but designators of natural entities, then to deny the legitimacy of the philosophical enterprise is to admit that there are realms of nature which should not be explored. This, obviously, requires justification.

(3) *The philosophy of science is a priori, and attempts to force science into preconceived forms; hence it is antagonistic to the method and spirit of science.* The obvious error in this objection is its assumption that the method of science contains nothing of the *a priori.* It seems clear that many notions in science function as relatively *a priori.*[11] In fact scientists are themselves divided into those who believe that the task of science is the anticipation of data through imaginative conjectures, and those who believe that it is the mere interpretation of data without benefit of preconceived hypotheses. The former, the *a priorists*, are those who follow Kant in believing that perceptions without conceptions are blind and hence that effective observation must be guided and directed by anticipated ideas, either inherent in the form of knowing or else merely suggested by anterior experiences. The latter, the *a posteriorists*, are those who follow Bacon in believing that all anticipation of nature is dangerous and hence should be abandoned in favor of a program of "listening to nature" without any preconceptions and without any hypotheses. The former insist that data are properly data only in view of hypotheses which determine their relevance; the latter reply that no hypothesis exists until the proper data have been observed. The former insist that hypotheses are the result of imagination, insight, intuition, or genius, and that they are therefore regulative over the data; the latter believe that hypotheses are read out of the data by a kind of scientific bookkeeping.

Essentially the same controversy occurs in the philosophy of science. On the one hand are the philosophical *a priorists*, not so numerous today among the philosophers of science as are their rivals, the philosophical empiricists. The former insist that many of the conceptions required by science cannot themselves be

[11] C. I. Lewis, *Mind and the World Order* (New York, 1929), Chap. VII.

derived by and through science. These basic principles must be presupposed by science, and can only be corroborated to a greater or lesser extent by the data of science. It is usually insisted, for example, that the principle that every event has a cause cannot itself be an inductive generalization. Russell proves that the principle of induction cannot be inductive.[12] Hence the task of science is essentially one of confirmation, not of discovery. But on the other hand there are many philosophical *a posteriorists*. According to this group all philosophical principles are inductive generalizations. The laws of logic as well as the propositions of metaphysics come under this characterization. The philosophy of science is then considered to be that enterprise which, upon the basis of a careful examination of science, is able to formulate hypotheses having explanatory value. The essential point is that the philosophical hypotheses are extracted from science, and are not obtained by *a priori* methods and then projected upon science as regulative conceptions. The philosophy of science "follows" science and is justified only in terms of its explanatory value.

The force of this objection, therefore, is weakened if one recognizes both that science is not necessarily inductive and that the philosophy of science is not necessarily deductive. The fact, of course, is that both science and the philosophy of science are complex disciplines which involve both the inductive and the deductive methods. The continuity of the disciplines, which has been argued throughout this paper, attempts to show that they are inductive and deductive in essentially the same way. There is no absolute *a priori* for either science or philosophy. But much that is relatively *a priori* for science is relatively *a posteriori* for philosophy. The recognition of this fact destroys the force of the argument just examined.

In conclusion, the exact character of the present discussion should be made clear. What has been proposed is a certain hypothesis which aims to explain the nature of the relation which the philosophy of science bears to science. What has not been made clear is the fact that this hypothesis cannot be philosophical

[12] *Aristotelian Society Proceedings*, 1935-6.

in the ordinary sense of the word since it is an interpretation of the relation between philosophy and science. Hence it cannot be a philosophical hypothesis for the same reason that the explanation of the relation of scientific hypotheses to scientific data cannot be scientific. It may therefore be called, if one desires a suitable terminology, a *meta-philosophical* hypothesis. And one might raise against this meta-philosophical hypothesis three objections: (1) It is too vague and speculative to warrant serious consideration. (2) It is merely a linguistic discussion and has no explanatory value. (3) It is *a priori* and attempts to force philosophy and science into a preconceived form. But if the thesis of this paper is correct the relation between meta-philosophical hypotheses and philosophical objects is the same as the relation between philosophical hypotheses and scientific objects, which is in turn the same as the relation between scientific hypotheses and scientific data. Hence one would expect precisely the same objections to be raised against the possibility of meta-philosophical hypotheses that were raised against the possibility of philosophical hypotheses. In fact the thesis of this paper is strengthened precisely by the fact that exactly the same objections arise at this new level; for the possibility of raising the same objections against two theses argues for the identity of the two theses at least in this respect, and it has been the attempt of this paper to demonstrate that identity. It seems safe to say, therefore, that the problem of the nature and status of hypotheses *in general* is a problem which continually emerges at new levels, hence is basic in any philosophical outlook.

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The Nature of Mathematics

BY

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THERE is a considerable divergence of opinion about the meaning of mathematics, and it is only with hesitation that I, as a theoretical physicist concerned mainly with geophysics, venture to discuss a matter that professional logicians differ about. Nevertheless I am concerned with the problems of the acquirement of knowledge by scientific methods, and the solutions of these problems involve pure mathematics, the validity of which one is usually willing to take for granted. But the logical schools are not so willing, and aim at demonstration. Logic must be true in all possible worlds, and all empirical propositions must be rigorously excluded. A scientist, on the other hand, is interested mainly in empirical propositions, and mathematics (with logic) is primarily a tool for investigating their relations. The proof of mathematics would therefore have interest for science, provided that the proof is sufficiently general to cover scientific requirements. If it is not, and the subject matter of the proof is restricted so that it fails to cover the scientific use of mathematics, then in its scientific application it is merely an analogy, and we are no better off than we should be if we simply took the validity of mathematics as it stands as a postulate.

The aim of the logistic school is actual demonstration of the existence of the entities treated. It is not enough to make postulates about number and develop their consequences, as the earlier mathematicians did; it is necessary to construct actual objects

that satisfy the postulates. A number is defined in Principia Mathematica as a class of similar classes. Russell remarks:[1] "We naturally think that the class of couples, for example, is something different from the number 2. But there is no doubt about the class of couples; it is indubitable and not difficult to define, whereas the number 2, in any other sense, is a metaphysical entity about which we can never feel sure that it exists or that we have tracked it down. It is therefore more prudent to content ourselves with the class of couples, which we are sure of, than to hunt for a problematical number 2 which will always remain elusive." Now it seems to me that the class of couples, far from being indubitable, certainly does not exist. If the analysis is to be valid the number 2 constructed in this way must be permanent; otherwise a proposition proved for one state of the world will have no application to another state. But if a pair of objects can become an individual or a triad the class of couples is altered. Married couples, one of whom may die, or who may produce a child, will invalidate the work unless further restrictions are introduced to exclude them. So will two drops of mercury, which may coalesce or one of which may break into two. These restrictions are not stated, and if they were stated it would be contradictory to say that Mr. and Mrs. Smith are a couple or that their number is 2, since by definition they would not be a member of the class of couples so restricted, and the number 2 would be this restricted class. It seems to me that Mr. and Mrs. Smith are a couple, and that the number 2, regarded as a description of a couple, also exists; but that the class of all couples does not.

A similar difficulty occurs in the definition of similar classes, by the establishment of a one-one correspondence between their members. For the criterion to have permanent value it is necessary that two classes, once similar, shall always remain similar, and this requires the permanence of individuals. If this is not universal, then if we have two pairs of classes, of numbers m and n, the combined classes used to define $m + n$ may

[1] Introduction to Mathematical Philosophy, 1924, p. 18.

not be similar by the time when they are compared, since in one or other of them individuals may have ceased to exist or been created. The similarity of classes is therefore not a logical concept but an empirical one; it is absolutely dependent on the permanence of distinguishable individuals. There may be such individuals, but it seems quite clear that there are many individuals that are not permanent, and then the whole treatment of similarity in terms of pure logic breaks down. Its scientific validity rests on the fact that classes once similar do often remain similar over long periods; but they do not always do so, and it is open to doubt whether they would ever do so if the interval of time was long enough. Thus a number, defined as a class of similar classes, will change with time; defined in terms solely of classes of eternal individuals, will exclude any class that contains non-eternal individuals, which will therefore have no number at all, even though at some specified date it may be compared with an eternal class and shown similar to it. But it seems entirely legitimate to abstract a common property of similar classes and call it a number, without making the false assertion that a class will always have the same number. Thus Russell's argument should be reversed; the class of similar classes has no permanent existence and is an impossible basis for mathematics; the use of a number as an adjective qualifying a class is legitimate, but its usefulness depends on the empirical fact that similar classes are often found to remain similar over long periods, and not merely on the fact they can be found at all.

An escape might be attempted through the fact that the sign ∃ of Principia, read as 'exists', is tenseless and might cover both has existed and 'will exist'. Then it would not be necessary to restrict our couples to those that exist now, and the class of couples might cover all that have ever existed or will ever exist. But this introduces a new difficulty. To say that Brutus and Cassius were a couple requires the existence of Brutus and Cassius separately, and this is not a matter of direct knowledge but an inductive inference based on the analysis of historical documents. The possibility of establishing a one-one correspondence between the members of couples that did not exist at the same

time is genuinely doubtful; it cannot be done by a physical process, and as we cannot identify the couples existing at any time but our own it is impossible to give a logical rule either. (Couples not observed simultaneously, though existing at the same time, are in little better case; their comparison also involves induction.) The class of all couples is therefore a thoroughly metaphysical notion and is of no scientific use; and a definition of number that depends on it is of no use to science, since it requires the problem of scientific inference to be solved first.

Again, on the constructional treatment of Principia the number n will not exist unless there is at least one class of number n. If the number of individuals in the world is finite, all finite numbers beyond this number are the null class and are therefore identical in this treatment, and mathematics as usually understood fails for them. *A fortiori* there can be no infinite classes. If n is the number of individuals, where n is finite, all classes of individuals numbering more than n involve contradictions; all classes of individuals numbering more than 2^n involve contradictions, and there will be no infinite class of any finite type. There are at most $n + 1$ natural numbers of classes of individuals (0 being included), and less than $(n + 1)^2$ rational numbers. The device for defining real number fails. Thus $\sqrt{2}$ is defined in Principia as the class of rationals whose squares are less than 2. But if the rationals are finite in number, any segment of them is completely determined by its highest member, whose square cannot be 2. Hence the whole theory of real numbers and continuity breaks down. To construct it we must have the axiom of infinity, that there is no largest finite number, or that if a class of number n exists, a class of number $n + 1$ also exists. If this is true, logic must take it as a primitive proposition. But it involves the constitution of the world, and its introduction implies either that the requirement that logic must be true in all possible worlds has been dropped, or that mathematics is not part of logic.

The question is not purely academic. Eddington's analysis appears to have shown that the total number of primitive particles in the universe is theoretically at most $2.136.2^{256}$; there are, I gather, some doubts about the validity of the proof, but I am

not concerned with them here. It is enough for the present purpose that the number may be finite. Dirac has given inductive reasons for supposing that the actual number is of order 10^{78} and is increasing. Even if we do not restrict individuals to be particles and allow them to be possible states, it remains perfectly possible on the quantum theory that the whole number may be finite.

According to Ramsey's analysis,[2] which rejects the axiom of reducibility but admits propositional functions in extension, the axiom of infinity is not an empirical proposition, though it cannot be proved; but this analysis is doubtful. The proposition "There are at least two individuals" is stated to be

$$\exists(x, y) : x \neq y$$

which "is the logical sum of the propositions $x \neq y$, which are tautologies if x and y have different values, contradictions if they have the same value.[3] Hence it is the logical sum of a set of tautologies and contradictions, and therefore is a tautology if any one of the set is a tautology, but otherwise a contradiction. That is, it is a tautology if x and y can take different values (i.e. if there are two individuals), but otherwise a contradiction. A little reflection will make it clear that this will hold not merely of 2, but of any other number, finite or infinite." Ramsey's reference to 'Wittgenstein's profound analysis' suggests that the argument is due to Wittgenstein, but actually in Wittgenstein's analysis $x \neq y$ is an impossible statement. Difficulties in connexion with the Principia definition of identity have led Wittgenstein to adopt the convention that a thing shall have only one name. $x = y$ is not a propositional function of the things denoted by x and y; "to say of two things that they are identical is nonsense, and to say of one thing that it is identical with itself is to say nothing."[4] Similarly to say of two things that they are not identical is nonsense, and to say of one thing that it is not identical with itself is impossible. Ramsey, however, while objecting to

[2] The Foundations of Mathematics, 1931, 60.

[3] $x \neq y$ means "x is not identical with y."

[4] Wittgenstein, Tractatus Logico-Philosophicus, p. 139.

the Principia definition of identity, considers Wittgenstein's convention hopelessly inconvenient (p. 51), since it prevents $x = y$ from being used as a propositional function in defining classes. He therefore retains $x = y$, but interprets it as made up of two predicative functions, of which if in fact x and y are identical, $x = y$ is taken to be the tautology

$$(\phi) \therefore \phi(x) .\mathbf{V}. \sim \phi(x) : \phi(y) .\mathbf{V}. \sim \phi(y)$$

and if x is not identical with y, $x = y$ is taken to be the contradiction

$$\exists(\phi) \cdot \phi(x) \cdot \sim \phi(x) : \exists(\phi) \cdot \phi(y) \cdot \sim \phi(y)$$

Thus $x = y$ has been replaced arbitrarily by a tautology or a contradiction; and it is of the negations of these tautologies and contradictions that Ramsey is speaking when he says that $x \neq y$ is a tautology or a contradiction. As far as I can see there is no objection to regarding the statement that there are at least n things in the world as an empirical proposition, true or false, and the contrary conclusion as the result of a fallacious definition. If Wittgenstein's convention and the Principia definition of identity are both rejected, $x = y$ should be regarded as an empirical proposition.

Wittgenstein's analysis[5] is purely verbal: "Logic fills the world; the limits of the world are also its limits. We cannot therefore say in logic: this and this there is in the world, that there is not. For that would apparently presuppose that we exclude certain possibilities, and this cannot be the case since otherwise logic must get outside the limits of the world: that is, *if* it could consider these limits from the other side also". I cannot claim to understand this passage, and I doubt whether it will bear Ramsey's interpretation. I should have said that *if* the limits of the world are those of logic, then logic can say what there is in the world; but Wittgenstein says that it cannot, and I should agree; while Ramsey apparently concludes that it can. The proper conclusion, I think, is that logic transcends the limits of the world,

[5] Loc. cit. p. 149.

and this is precisely why I consider that the constructional method was foredoomed to failure. We must be able to consider the consequences of false empirical hypotheses before we can say that they are false, but the constructional method makes it impossible to consider them at all.

Before leaving the matter, however, we may notice what the consequences of rejecting the axiom of infinity would be. At first sight it might appear to have very little. If the number of individuals is in fact n, we shall never need to count more than n, and the pure mathematics of numbers up to n may still be valid; the only parts of pure mathematics sacrificed may be those that are never used. But in fact we sometimes go outside the range to obtain results valid within it. Thus 2^{1000} is a natural number and may be the number of some class even if there is no infinite class. But we do not evaluate it by direct multiplication; we work it out, to the accuracy we require, as $10^{1000 \log_{10} 2}$. But $\log_{10} 2$ depends on summing two infinite series or evaluating two integrals, and making Dedekind sections; the exponentiation needs another. 2^{1000} might still exist and the practical means of evaluating it be invalid. For the factorial 1000!, which we should usually estimate by Stirling's theorem, the same difficulty arises.[6] To justify these methods, if the axiom of infinity is false, we need to show that it nevertheless does not lead to wrong results within the range where correct results exist. This is perfectly possible. It is sufficient that the mathematics that assumes it should be self-consistent. Principia has developed the consequences so far beyond the limits needed for scientific use, without finding any inconsistency, that it is reasonable to suppose consistency within these limits; though, according to Gödel's theorem, if consistency could be proved it could also be disproved, and therefore if the system is consistent a proof of consistency is impossible. Then the *use* of infinity, distinguished from the axiom of infinity, is merely a convention, a device for obtaining results that could be obtained without it, though with more labour. It is necessary to comment,

[6] A practical computer checks his work by working out his results by different methods. Thus induction is used to test the correctness of what is meant to be deduction.

however, on the proposition of Principia read as 'a false proposition implies any other'. If this was a correct reading, and the axiom of infinity was false, it could lead to any result whatever. But the reading is incorrect. The proposition is 'if p is false, then it is not true that p is true and q false'. This is an evident tautology, but it has nothing to do with q, and should not be read as 'if p is false, then p implies q'. The fact that if p is false it would also imply $\sim$ q is enough to show that the Principia meaning of 'implies' does not express a relation between p and q at all; the definition of 'p implies q' is simply 'it is false that p is true and q false', which holds either if p is false or q true, quite irrespective of any relation of deducibility between them. I fully agree with Lewis and Moore that the use of 'implies' should be restricted to the latter. It is perfectly possible, if the axiom of infinity is in fact false, that we shall be able to deduce contradictory conclusions only by assuming it true and false in the same proof. If we consistently assume it true, and it happens to be false, we may still get the right results within the range of numbers that exist.

These considerations refer only to the technique of computation. The axiom of infinity is involved in scientific laws in a more subtle way. Consider the theorem of Pythagoras and its consequence, that the hypotenuse of an isosceles right-angled triangle is $\sqrt{2}$ times the base. In a finite mathematics $\sqrt{2}$ does not exist, and we must either sacrifice the theorem of Pythagoras or the existence of the isosceles right-angled triangle. Now here we must distinguish carefully between the branch of pure mathematics called geometry and the experimental science of mensuration. About the former I have nothing to say. But mensuration can make either sacrifice without the slightest difficulty, once we recognize the essential nature of scientific laws and abandon the false belief that they deal with exact causality. Apart from simple counting of individuals no scientific law is an exact statement,[7] and even in this case there may be failures that we should describe as due to the creation or impermanence of individuals. In all other cases, however accu-

[7] Jeffreys, Proc. Aristot. Soc., 1937, 61–70; Nature, 141, 1938, 672–676, 716–719.

rately we try to repeat the conditions, there is an unexplained outstanding variation. The statement that scientific laws are exactly verified is simply false. In my 'Scientific Inference' I originally adopted the customary separation of an observed value into a true value and an error, the former being supposed to have in some sense an independent existence. (This is corrected in the 1937 addenda.) The correct attitude is that the law deals with the variation as a whole and with the probabilities of variations of different amounts. Every law is provisional in the sense that its parameters are capable of revision with additional data, and even that new parameters not yet considered at all may be introduced when there is adequate evidence to support them; some of these parameters may represent a variation according to an exact formula and the others random departures from it, but from an epistemological point of view the random variation is just as much a part of the law as the systematic variation is. Now experiment will never say that the ratio of two sides of a triangle is exactly $\sqrt{2}$ and not a rational fraction sufficiently close to it. It can at most determine each side with a certain margin of error, and as there are rational fractions indefinitely close to $\sqrt{2}$ we can always find one that agrees with $\sqrt{2}$ within this margin. Nor can it say that any particular triangle is exactly an isosceles right-angled one, and if we admit that it may not be, it is perfectly possible without abandoning the theorem of Pythagoras to suppose that the ratios of the sides are rational. There is one complication from the law of error; since the normal law of error involves the exponential function it depends on the axiom of infinity. But the normal law is not an exact statement, and could be replaced by a set of superposed binominal distributions with ranges in rational ratios, and the final readings are all rounded off to the nearest scale interval. Thus the normal law of error may well be replaced by a law that permits only a finite number of values. There is no experimental proof of the so-called 'geometrical intuition of continuity'; the data could be described, within the uncertainty, in terms entirely of the relations between finite numbers. We could abandon the hypothesis of continuity in the mathematical

sense, according to which the limiting point of any subset belongs to the set; we could even abandon the belief that there are an enumerable infinity of points on a line. The consequences would be strange, certainly. If two lines cross, for instance, they need have no common point; the intersection might be in a gap. But since a physical line is composed of a finite number of molecules this may very well be true if we do not regard unoccupied points of space as individuals. The difficulty is not one of fact but of epistemology. The hypothesis of a finite number of points on the line requires the introduction of a parameter to express their separation, while that of continuity does not. The latter is therefore a possible trial hypothesis, while the former would need special evidence.[8] For scientific purposes continuity must be considered before discreteness.

However, if we grant the premiss that logic and mathematics are to be true in all possible worlds, and find that large pieces of them depend on the constitution of the actual world, that is not merely a blemish, as the authors of Principia and Ramsey admit, but a *reductio ad absurdum*. The trouble is indeed that these works do not deal with logic alone, but general statements about the world have been put into their very foundations. Otherwise the definition of number has no meaning, whether it rests on propositional functions in extension or the axiom of reducibility. It is impossible to make a statement about the number n until a class of number n is actually found; thus on these systems mathematics is empirical and inductive, which again contradicts the intention of building up a purely deductive system. For scientific purposes an inductive system is necessary; but the essence of an inductive system is that it must be willing to assimilate new information. It would be impossible to base an inductive system capable of finding out that the number of individuals is 2^{1000} or more on a deductive one that denied *ab initio* that the number was more than 2^{500}; and if the deductive system said that the number was at least 2^{1000} it would assert *a priori* that the number of individuals was at least this, if we accept the logistic definition of number, and would deny the

[8] Jeffreys, Proc. Roy. Soc. A. 162, 1937, 487–491.

possibility of finding out by inductive methods that it was less. I conclude that for scientific purposes the objective definition of number and the axiom of infinity must be totally rejected, because they make *a priori* statements about the world that could be made, if at all, only on the basis of experience. The function of mathematics and logic is not to decide what is possible, but to provide a framework of sufficient generality to include all possibilities. Their propositions will be of the form: if x has the property ϕ, it will have the property ψ. They will not say that an x exists and has the property ϕ, and cannot therefore assert that an x has ψ. That is a matter for experience. The assertion sign cannot be attached by logic to any implied proposition. It might appear that if $x = 1$, we can assert $x^2 - 3x + 2 = 0$; but this is only because we have attributed to x the characteristic properties of number, which must therefore not be forgotten in any conclusion that we draw. We are entitled to consider any presupposed properties that we like, and to develop their consequences, but we can discard them only if they are found self-contradictory. Curiously, this was the attitude of pure mathematicians themselves before mathematical logic tried to justify them. On such a view of the nature of mathematics, however, there will be large fields with no scientific application; only some of the pigeon-holes provided by our generalized mathematics will be occupied by scientific propositions. We can consider the consequences of infinite aggregates without either asserting or denying their existence, leaving it to experience to decide whether any particular aggregate is infinite or not.

Inductive inference is necessarily in terms of probability, but there are two possible attitudes to the nature of the relation between deduction and induction. They may be of the same character, deduction being merely the extreme case of induction. There has been no attempt at a formal development of this attitude. The more usual is that induction involves a special notion, probability, which obeys certain relations that permit its numerical representation, and can therefore be treated by the methods of pure mathematics. Its processes can be formally stated, and they have the requisite generality: that is, given any

set of hypotheses, it is possible to make a decision between them on any observed data, or to say that the data do nothing to discriminate between them. Any data can be assimilated into the scheme. (This is not true of the frequency definitions of probability, which attempt to define probability in terms of the constitution of the world and are open to the objections urged above to the Principia definition of number, besides others of their own.[9] If the theory of probability is to cover the content of science, and not to lead to illegitimate inferences, probability must be taken as an undefined notion.) If pure mathematics is taken in the generalized sense, then, the procedure is valid. It is self-consistent in the sense that the final probabilities of a set of hypotheses are the same in whatever order the data are taken into account.[10] In 'Scientific Inference', however (Appendix I), I found a difficulty associated with what C. I. Lewis calls the 'peculiar propositions' of Principia. The question is whether the 'certainty on the data' of the theory of probability is identical with 'material implication' (called simply 'implication' in Principia). I was under the impression that it was, but this cannot be maintained. The only contradiction that I deduced from the identification with material implication was

$$P(q \,.v.\, \sim q \mid p \cdot \sim p) = 2$$

which is a contradiction, since by the conventions of the theory no probability can exceed 1. This appeared trivial, since in practice p and $\sim$ p cannot appear among the data simultaneously. But the matter is more serious. Consider the proposition 'p implies that q implies p'. This, correctly read, means: 'if p is true it is not true that q is true and p false'—an evident tautology. But on the suggested interpretation it could be read as 'if p is true, the probability of p on data q is certainty'. This is obviously nonsense. The probability of p on data q has one value and only one, and this is quite irrespective of whether p is true or not. As an example, the proposition would say that if a coin

[9] Wrinch and Jeffreys, Phil. Mag. 38, 1919, 715–731; Jeffreys, Scientific Inference, 1937, 218–222; Phil. Mag. 32, 1936, 337–359.

[10] Proc. Roy. Soc. A., 165, 1938, 190–192.

has been thrown ten times and given ten heads, then it would certainly give ten heads in succession on the data that it is unbiased. Here P(p|pq) = 1; but we cannot detach the second p and say that if p is true, P(p|q) = 1. If we use the information that p is true we are assessing P(p|pq), not P(p|q).

Similarly the proposition 'not-p implies that p implies q' means that 'if p is false it is not true that p is true and q false', an evident tautology whatever q may be. But if it is read as 'if p is false, q is necessary on data p' we again have nonsense, for the same reason as before. This would say that if a coin is biased, then it would be certain to give ten heads in succession on the data that it is unbiased; and of course also certain to give ten tails. It is clear, therefore, that certainty on the data, as understood in the theory of probability, is not the same as material implication.

The question is of the first importance in the problem of learning from experience, because if 'certainty on the data' meant implication in the sense of Principia, then any observed data whatever could be regarded as confirming a false hypothesis, and there would be no valid induction. 'q is certain on data p' cannot be read as meaning simply 'it is not the case that p is true and q false' or 'not-p or q'. Now C. I. Lewis and G. E. Moore have both recognized the need for the more restricted meaning of implication, and have introduced the terms 'strict implication' or 'entailing' for the relation between two propositions when one is a consequence of the other.[11] Lewis defines 'p strictly implies q' as meaning 'it is impossible that p should be true and q false', impossibility being taken as a primitive notion different from mere falsity. 'p is impossible' is denoted by $\sim$ p, 'p is false' by -p. Now what can 'p is impossible' mean? *A priori* the only impossible propositions are contradictions. But if p is a significant proposition it can be declared false or impossible only on evidence not contained in the propositions of logic. Thus 'p is impossible' is a legitimate statement only if p is a logical con-

[11] Lewis, Mind 21, 1912, 522–531; 23, 1914, 240–247; Survey of Symbolic Logic, 1918. Moore, Proc. Aristot. Soc. 20, 1919, 40–62; see also D. J. Bronstein, Mind 45, 1936, 157–180.

tradiction or implies contradictory results when combined with logical principles. If p is a significant proposition 'p is impossible', in Lewis's sense, is always false. But 'p is impossible on data r' may be a legitimate statement if r itself is a significant proposition. Then we must distinguish our cases; 'p is impossible on data r' will have different truth-values, both according to the proposition substituted for p, and that substituted for r. A caution is therefore needed about the meaning of 'p strictly implies q'. It is defined to mean that on the rules of logic alone, it is impossible that p should be true and q false; and if p and q are both significant propositions it is not impossible on the rules of logic that p should be true, and equally it is not impossible that q should be false. Thus 'it is impossible that p should be true and q false', for significant propositions, is equivalent to 'q is deducible from the rules of logic together with p'. The latter is what Moore calls 'entailment'.[12] Further, this corresponds to the needs of epistemology. We might know q directly; in that case p is irrelevant to our knowledge of q. But if we know p and can show that p and logic together entail q, we can assert q, and in that case the logical process yields new knowledge.

Now Lewis's proposition 3.55

$$\sim -p \prec (q \prec p)$$

must be read, in terms of the definition, 'if p is necessary, it is impossible that q should be true and p false'. If the data are purely logical, p, to be necessary, must be a tautology, and it is impossible that p should be false; hence q has nothing to do with the proposition. But if we have also significant data r we can assert, for significant values of p and q, 'if p is necessary on data r, it is impossible that q should be true and p false on data r'. This again is obviously true, and q again has nothing to do with it. But we cannot read the proposition as 'if p is necessary on data r, then it is impossible on the rules of logic alone that q

[12] 'Deducible' should, I think, be read as 'deducible by a perfect reasoner.' The actual reasoner, deductive or inductive, differs to some extent from perfection. The difference can be illustrated by the propositions 'the properties of numbers entail Fermat's last theorem'; 'Mult. Ax. entails $C = \aleph_1$.'

should be true and p false'. This would be to use the sign $\prec$ in two senses in the same proposition. The tautology arises, not from q implying p, but from r implying p.

Similarly Lewis's 3.52 runs

$$\sim p \prec (p \prec q)$$

and must be read 'if p is impossible, then it is impossible that p should be true and q false'. This requires p to be a contradiction, and it is impossible for a contradiction to be true; thus we have a tautology independent of q. With the extended meaning 'impossible on data r', the proposition is still a tautology; but it must not be read as 'if p is impossible on data r, then q is necessary on data p'—or even, 'then q is necessary on data pr'. The point is that if p is impossible on data r, then it is impossible that p should be true on data r. p does not imply q in any sense whatever; what happens is that r implies -p, and q is irrelevant.

The error in speaking of the probability of a proposition as a function of the proposition alone, without reference to the data, was exposed by Bayes in his classical paper of 1763, and to neglect the distinction is to deny the possibility of learning from experience. The characteristic notation P(p|q) of the theory of probability is introduced to allow for it. It appears that the point is worth attention even in the extreme cases of impossibility and certainty. Thus for significant propositions we could write 'p is impossible on data r' as $\sim_r p$. Then $\sim_r (pq)$ will say that p and q cannot both be true if r is true, and therefore either p is impossible on data r or q is impossible on data pr. Hence

$$\sim_r(pq) : \equiv : \sim_r p \,.\mathbf{V}.\, \sim_{pr} q.$$

This will be the special case of the product rule for probabilities

$$P(pq \mid r) = P(p \mid r)\, P(q \mid pr).$$

where both sides vanish. Note that if r is replaced by the propositions of logic and p is a significant proposition, $\sim p$ is false and we have

$$\sim (pq) \cdot \equiv \cdot \sim_p q$$

and strict implication is equivalent to entailment.

If p is not a significant proposition, but either a tautology or a contradiction, the propositions still do not entitle us to assert that 'a contradiction entails any proposition' or 'any proposition entails a tautology. A tautology is in fact implied by the laws of logic alone; all that we need to say is that additional data consistent with logic do not invalidate the implication.

Whether a contradiction entails any proposition is doubtful. It would appear that if we allow contradictory propositions to appear in the data simultaneously we shall in general be able to deduce other pairs of contradictory propositions, but it does not appear obvious that every proposition will be accompanied by a contradictory one, though it may be true. Thus if x is taken to be both 1 and -1, all rational functions of x^2 will have the same values in both cases; but $x^2 = x\,(x)$ and can be taken to be $1\,(-1) = -1$ if x is taken to be *both* 1 and -1. The problem is best dealt with, I think, by the convention that contradictions must not be allowed to appear in the data.

The conclusions that I should draw are therefore:

(1) The definition of number in Principia Mathematica rests on an inductive inference, and the claim that mathematics has been shown to be a part of logic is not justified.

(2) The axiom of infinity, as stated in Principia, does not even rest on an inductive inference, and is very probably false.

(3) The constructional approach to mathematics essentially involves a circular argument, since it depends on the assertion of empirical propositions, which in turn require the theory of probability and mathematical technique to establish them.

(4) It is legitimate to develop mathematics as the consequences of a set of postulates, assuming the rules of logic, without asserting that there are in fact things in the world that satisfy the postulates. The peculiar propositions of mathematical logic are tautologies when read in terms of the Principia definition of implication; their analogues in Lewis's system of strict implication are also tautologies in terms of the definition. But in neither case do they justify the usual readings 'a true (or necessary) proposition is implied by any other' or 'a false (or impossible) proposition implies any other.'

(5) If p and q are significant propositions, 'p strictly implies q'

in Lewis's sense is equivalent to 'p entails q' in Moore's; and the latter is identical with 'q is certain on data p' or 'P(q|ph) = 1' of the theory of probability, where h is understood to include the axioms of the theory.

(6) If p is a tautology, it is strictly implied or entailed by the laws of logic and leads to no difficulties. If p is a contradiction, or leads to contradictions, it will in general be possible to deduce other contradictory conclusions from it, though there is no conclusive reason to suppose that for every proposition q deducible from p the negation $\sim$ p is also deducible; the best way of disposing of such difficulties is to make the convention that contradictory propositions shall not appear in the data. The adoption of the postulational treatment of mathematics, suggested in (4), will not lead to contradictions even if there are no things in the world that satisfy the postulates. It will lead only to a set of implications whose premisses are unasserted, and science can use observed data to select what premisses can be asserted, thus selecting from the body of logically valid mathematics those parts that have applications to the actual world.

The difficulty introduced: by the existence of impermanent individuals into the constructional treatment of number has already been pointed out by Dr. N. R. Campbell in his 'Physics,' and in a paper read at the summer meeting of the Aristotelian Society this year. In the subsequent discussion Russell called attention to the fact that in Principia all propositions that depend on the axiom of infinity are stated in the form 'the axiom of infinity implies so-and-so.' It is not necessary to assert the axiom of infinity and therefore not necessary to assert its consequences. This goes a long way towards meeting the difficulties mentioned in this paper. If the axiom is only considered and not asserted, suggestion (4) is satisfied.

In some parts of this paper I have been considerably helped by discussion with Prof. G. E. Moore.

In a discussion at the summer meeting of the Aristotelian Society, after this paper was written, Russell called attention to the fact that propositions in *Principia* that depend on the axiom of infinity are

stated in the form 'the axiom of infinity (and the other axioms of Principia) imply so-and-so.' The detachment of the postulates and the interpretation of the propositions as 'the axiom of infinity and the other axioms of Principia are true, therefore so-and-so' is a separate step and need not be taken. This meets a large part of my criticism, which concerns the second step. If it is not taken, the claim that entities satisfying the properties of numbers have been discovered in the actual world is abandoned. But if it is possible that a world satisfying the axioms could exist, irrespective of whether it does exist, that is enough to establish that the consequences of the axioms are consistent, which is enough for my purpose; and I should say that such a world is a possible one, though not the actual one.

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The Future of Theoretical Physics

BY

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INTRODUCTION

THE modern theoretical physicist is a strange creature. Perhaps in every age the theoretical physicist has seemed so to his contemporaries who took the trouble to observe and think about him. There seems, for example, something almost too magical in the uncanny way in which he pulls a brand new atomic particle out of a theory, a way that reminds us of a rabbit being produced out of a hat. This is usually done in an off-hand manner with a considerable amount of patter of a highly abstract symbolic character but with a few words inserted in the right places for the benefit of the experimental physicist and a final prediction that if the experimenter does thus and so in the laboratory he will find a certain result. No wonder the observer marvels when the answer comes out right. But as he thinks further he is inclined to wonder whether it is necessary or desirable for the details of the process which led to the result to be so unintelligible. For unintelligible they seem to be to a great many people who are directly concerned with their understanding, to the people who for years have been laboring under the belief that they are engaged in the task of finding out how Nature works. What is this curious process whose whole task, according to one distinguished exponent of it, is merely to predict experimental results, and what is its future

likely to be? These are the questions we are setting for ourselves in the present discussion. It is unlikely that the answers given here will satisfy everybody, but it is reasonably certain that nobody who aspires to an interest in science can fail to attach some meaning to them whether or not he agrees.

One of the pathetic symbols of our day is that scientists should have so much trouble in understanding each other when the accepted meaning of science is the systematic organization of knowledge. We are all familiar with the effect of over specialization on the meetings of scientific societies. In the American Physical Society, for example, it is rare to find many who can follow the speakers in the theoretical sessions. We have in physics perhaps not reached the stage which characterizes most mathematical meetings in which only one man is presumed to know what is going on. (We hear that there is occasionally doubt about even this.) When most of us want to find out what the theoretical physicist is *really* doing we have to go to the so-called popularizer, who endeavors to understand it all and translate it for us into language we can understand. The results are often disturbing and this is particularly apt to be the case when the theoretical man himself turns popularizer. We are then confronted for instance by statements to the effect that physics has abandoned the principle of causality, that modern physics makes God appear more and more in the light of a pure mathematician, that "there is an absolute limitation of the applicability of visualizable conceptions in the description of atomic phenomena." In a somewhat different field, we hear that the Universe is expanding, but that it is finite, yet unbounded, and that it contains an infinite number of objects. Another celebrated authority states that he has counted the number of objects in the Universe (protons and electrons) and finds that the number comes out to be $2\times136\times2^{256}$. (The presence of the mystical number 136 makes it unnecessary to reveal the author of this venture into the higher arithmetic.) Small wonder the newspaper editors continually harp on the unintelligibility of modern theoretical physics and quote with approval Einstein's saying that it consists largely of "drawing one incomprehensible out of another

incomprehensible." God forbid that it should reach the stage already reached by philosophy and become in the language of one scientist, "the systematic misuse of a terminology specifically invented for that purpose."

Now it may well be objected that we are not being fair to the popular expositors of theoretical physics in selecting isolated statements from their books and articles; that before passing judgment on their attempts to make clear the current trends we should listen carefully to *all* they have to say. We should then presumably understand statements like the above in their proper contexts. But even if we have time to carry out this more ambitious program we are baffled by a serious difficulty: the authorities do not agree as to the meaning of what they are popularizing. The few examples just given would lead one to suspect this and a careful reading of popular and semi-popular expositions only confirms the suspicion. In this matter we need not feel particularly concerned about the general public, who may be expected to view the disagreement of the physics "doctors" with amused unconcern. But can we as scientists be satisfied with this attitude? Should we say: what difference does it make whether we understand what theoretical physics is all about or how it is likely to develop in the future? After all, is not what matters most that people in Schenectady should make better vacuum tubes and people in New York better microphones? Are these eminently practical matters likely to be seriously affected by a difference in point of view with regard to the nature of physical theory, for example, in which one side insists after lengthy investigation that the fundamental principles of physics all come out of our heads and are purely *a priori* while the other side thinks this view is ridiculous and that physical knowledge is definitely *a posteriori*, i.e., known only as a direct result of experience? Now leaving aside the purely esthetic interest in such questions which a certain type of mind can not help feeling, we are continually being reminded even by those whose knowledge of physical theory is not very great that the theory of today becomes the practical application of tomorrow. Therefore it is inconceivable that we can afford to neglect an appreciation of recent

trends in physical theorizing. In particular we shall have to note the important rôle which the physicist's attitude toward the fundamental hypotheses of his theories has played in the past and will presumably continue to play in the future.

It is clear, however, that we are not going to get very far in our appreciation unless we go back to the beginning and ask ourselves what physics really is. If we pursue this line we shall have reason to see that the distinction between theoretical physics and the rest of physics becomes illusory and that the future of theoretical physics is really and genuinely the future of physics itself.

WHAT IS PHYSICS?

Most physics texts contain on page 1 a statement, usually discretely vague, which passes for a definition of the subject. Most instructors pass over this with a certain amount of impatience in order to get down to the real business of learning what comes on pages 2, 3, 4, etc. One can hardly blame them, for these statements which often manage to bring in such grandiose words as Universe, laws of Nature and the like, are essentially meaningless. Anyone who has ever tried to frame a definition of physics in a few words realizes that it is not a simple matter and that most attempted definitions are merely "stunts" which only serve to focus attention on one or another aspect of the subject.

It is much more sensible for our discussion to say that physics is an activity of the physicist and then to proceed to a brief outline of that activity which reduces essentially to an attempted description of a portion of human experience.

In amplifying this statement let us note in the first place that the physicist contents himself with a very limited portion of experience which we take here to be synonymous with his sense impressions. The latter we shall not analyze too carefully for fear of getting mixed up with more purely philosophical complications. But the curious thing is that he is not content with taking even these limited sense impressions as they come to him without effort on his part. He insists on obtaining new ones by

a process called *experimentation*, involving conscious activity. This is not the place for a detailed analysis of what an experiment is, but there are certain features which must not be overlooked. It implies a definite increase in experience and hence of knowledge over what we might expect if we merely sat around waiting for something to happen. Moreover we must not forget that the very way in which we carry out the experiments conditions the kind of knowledge we are going to get. Many popularizers are continually emphasizing that in science we want to know the *truth* about *Nature*, as if Nature were something quite apart from us. Actually, of course, Nature is simply what happens when we perform our experiments and a radically different method of performing experiments would lead to knowledge of a quite different Nature. It may be objected that there are definite limitations to the way we can do experiments, but no one knows yet precisely how far these limitations go. We proceed by bringing things we call physical objects in juxtaposition and observing what happens which does not happen while they were apart. The number of possible combinations of this kind though finite is so very large that we may feel fairly confident we shall not complete the trial of them all for a very long time. Moreover, we must not forget that the physical science of our day is still committed deeply to the conviction that experiment must be quantitative, i.e., involve *measurement*. This in turn means that all physical experimentation is founded on certain geometrical concepts, since ultimately all measurement reduces to attaching a number to the coincidence of a pointer and a scale. At one time it was thought that a single geometry, namely Euclidean, is imposed of necessity on the human mind, so that in all measurement its use is *a priori* inevitable. This view has since passed into the discard and we are now all familiar with non-Euclidean geometry and its use in relativity. It is probably not sufficiently appreciated that as the human organism evolves the mind may ultimately find it convenient to utilize a non-Euclidean geometry as the basis of all physical measurements. Certainly if this happens Nature will appear to our descendants in a quite different guise from that in which it appears to us.

Now physics might very well consist of a verbal description of our experiments in terms of everyday language plus the numbers we obtain in our measurements. As such, to be at all precise it would of necessity be very verbose. Moreover, though the description of the operations performed in doing the experiments would not provide essential difficulty since diagrams can be used to provide concrete representation of the terms employed and to remove the danger of ambiguity, a wholly different situation is met in the series of numbers by which the pointer coincidences are represented. Here arises at once the fundamental problem of interpretation: we desire to attach *meaning* to the results of measurement. Why do we want to do this? This is a philosophical or psychological question (the word "why" ought strictly to be banished from physical discussions, but mankind, though appreciative of logic, is never really logical) and hence strictly speaking not fit to be entered into here. However, we may suppose (transgressing the rule) that the reason we do not consider we have done enough in merely *tabulating* the results of measurements obtained by various operations is that: 1) we find that on *repetition* of certain operations we obtain results which are *nearly* the same, thus emphasizing that this portion of our experience is not chaotic but displays a certain *order;* moreover a little further examination persuades us to the belief that many kinds of operations are not so isolated, so intrinsically different as the way we carry them out and the way we describe them in ordinary language would appear to indicate. We are therefore led to seek *relations* among apparently different groups of operations. At this stage we are definitely on the way to theorizing: our physical description takes on a more abstract and sophisticated character. An illustration will make this clearer.

Suppose we go through the operation which is called measuring the volume of a gas contained in a vessel provided with a piston, and for each value of the volume also measure the pressure, keeping the temperature constant. A complete description of this affair would be somewhat lengthy and may safely be omitted. The result is a series of numbers which we call corresponding

values of pressure and volume at constant temperature. The question is: what shall we do about them? We might do nothing, but actually there is an inner compulsion forcing us to see what happens when the experiment is repeated using perhaps a different gas and a different (though probably not too widely different) temperature. We thus get various sets of comparative numbers and a very cursory examination discloses the fact that as the pressure numbers go up the volume numbers go down. This is found to be the case for all series of observations. We conclude we have discovered a routine of experience. How shall we describe it? We might leave the description as it has just been given. But this seems unhandy and uneconomical. As long as we are using numbers we may as well employ the other dodges which mathematics suggests and describe the measurements by the introduction of algebraic symbolism and the concept of functional relation. It must be noted that as soon as we do this we are far transcending the original brute experience involved in the actual operations. Already we are making hypotheses fundamental for the whole course of physical theory. There is no *a priori* guarantee whatever that the introduction of the symbol p to denote the result of carrying out the operation which we call measuring pressure and the introduction of the corresponding symbol V for the volume and finally the treatment of p and V as variables subject to all the conventional rules of the game of mathematics will provide a successful description of physical experience. In itself the decision to take this step was a bold experiment, however natural it may seem to us today due to the familiarity produced by an educational process which almost forces us to look at matters in this way whether we wish to or not!

The functional relation between p and V with which we thus decide to represent the results of many such series of operations on gases then becomes a *physical law* (the familiar one known by the name of Boyle), a symbolic and economical description of a routine of experience. Of course we pay a penalty for our apparent success: viz., all physical laws fail somewhere to be descriptive of experience. Thus we are familiar with the fact that Boyle's law certainly does not describe a routine of experience for

all gases under all conditions. All we can say from the viewpoint thus far under discussion is that it is a good description under certain rather closely limited circumstances.

We could continue to study from the viewpoint just illustrated various experimental operations and describe them in terms of laws. Would this constitute physics? It might but it actually does not satisfy. A mere collection of laws in symbolic form is doubtless more economical than a collection of verbal statements about physical operations. But it ignores completely the possibility of *relations* existing among apparently diverse operations. In the case of Boyle's law, for example, it overlooks the possibility of the existence of *other* operations to the results of which the symbol pressure can be just as appropriately applied as to the operation of noting relative levels of mercury in a U-tube. It ignores in short the possibility of subsuming large realms of phenomena by the introduction of a few fundamental concepts, transcending particular operations, and certain mathematical relations postulated to relate them. When we reach the stage of conceiving physical description in this guise we are ready for a *theory*. It marks the third and highest stage in the development of the method of physics. It is properly to be regarded as *metaphysical* since both the concepts and the hypotheses that relate them must of necessity go beyond the actually observed physical phenomena and the laws which serve as their symbolic description. To most people, perhaps one might almost say indeed to many physicists, this remains today in spite of more than half a century of discussion of the foundations of physics the least understood of all the activities of physical science. Yet the process is perfectly plain from a methodological point of view as can be seen by even a cursory inspection of the most famous of all physical theories, that of mechanics.

The theory of mechanics begins with certain primitive notions of space and time, notions too primitive for precise definition (like the undefinables of geometry) and proceeds to construct out of them precise concepts like displacement, velocity, acceleration, mass and force. These very definitions demand the introduction of postulates. Thus the logical definition of mass requires the

postulation of certain hypotheses about the mutual acceleration of pairs of interacting particles. These assumptions constitute the principles of mechanics of which the famous $F = ma$ is the most noteworthy and useful. We often call these "laws of motion" but they are not laws in the genuine methodological sense: they are the basic hypotheses of the theory. Though suggested by experience they can *never* be verified directly. From them however one can by purely logical processes derive laws like $s = \frac{1}{2} gt^2$ for a falling body, etc. In this sense they subsume in themselves all possible motions of bodies or at any rate we hope so. To the extent that this is true the theory of mechanics is successful. It is only because the basic hypotheses are so directly suggested by experiment that we have got into the habit of thinking of mechanics as somehow true in the self-evident sense. The history of physics contradicts this view and shows that mechanics is precisely like every other physical theory in its logical structure of primitive concepts, derived concepts, hypotheses and derived laws.

THE NATURE OF PHYSICAL HYPOTHESES, PAST AND PRESENT

We have just emphasized the building of theories as the final stage in the process of the description of that portion of human experience which we choose to call physical. A complete treatment of the nature of physical theory is impossible within the limits of our present discussion. But it is clear that the future of theoretical physics and hence of all physics depends on the nature of the concepts and hypotheses at the basis of physical theories and the way they are manipulated in the deduction of physical laws. This, of course, assumes that the fundamental *method* already outlined will continue to be used—that there will be no radical change in the nature of the description. If indeed measurement as we now understand it is abandoned and some other way of *ordering* physical operations is ultimately adopted, the whole procedure of physical description may become something quite unrecognizable by us today. The day may conceivably come when we shall decide to burn up our meter sticks, scratch out our scales and proceed to pay more attention to *shapes* than

to *sizes*. After all mankind has found other figures than the purely metric worthy of admiration! But speculations on this score, while amusing, are idle; as idle indeed as the speculations as to what the man of the future will be like if his nervous system continues to become ever the more important part of him. As far as the immediate future is concerned we shall have quite enough on our hands to consider the evolution which has taken place in the attitude of physics toward the basic concepts of its theories.

Let us spend a few moments on this historical development. As we look back over the history of physics we find naturally enough a tendency in early days to take as the fundamental assumptions of theories propositions suggested rather immediately by and closely connected with direct experience. This is very evident, for example, in Aristotle's physical theorizing, vague as it was. When Aristotle associated the concepts heaviness and lightness with the rough experiential tendency of certain bodies to fall toward the earth and others to rise, he was certainly inventing concepts close to observed experience, concepts that can be vividly grasped by the mind which does not like to stray far from the actual facts in its description. In emphasizing this point we naturally overlook Aristotle's lack of care in getting the physical facts straight.

We see much the same tendency to stick to the facts in the work of Archimedes, the greatest natural philosopher of antiquity. No one can examine his treatment of fluid statics without being clearly aware of this.

Galileo is by common consent considered the father of physical science as we now know it. Reading his "Dialogues concerning Two New Sciences," one cannot fail to be impressed with the insight he displays in the choice of concepts and hypotheses best fitted to describe observed motions. We find in him a perfect appreciation of the postulational nature of physical theory. At the same time he is anxious to give the postulates maximum *a priori* plausibility by phrasing them as closely as possible in terms of direct experience. A celebrated example is to be found in his discussion of freely falling bodies. Here he bases all his

theorems (cast, as was natural, in the geometrical form) on the postulate that the velocities acquired by the same body sliding down frictionless planes are equal when the heights of these planes are equal. He expresses his regret that he is unable to verify this assumption directly because of practical difficulties. Nevertheless he clearly feels himself impelled to try to make his fundamental postulate as plausible as possible on the basis of actual experience, for he discusses in detail an alternative but somewhat similar situation which can be more readily realized experimentally. This is the celebrated pendulum experiment in which the string is caught as it passes the vertical by a nail placed directly beneath the point of suspension. The bob, however, is observed to reach (if conditions actually allow it) approximately the same height as it would if allowed to continue without the constraint. It is a tribute to Galileo's genius that he realized the close connection between these two situations, which today we find it most convenient to express in terms of the concept of energy that Galileo did not have at his disposal. However, from our present standpoint of the examination of the nature of physical hypotheses the most interesting feature of the illustration is his feeling that although one must invent concepts and make hypotheses concerning them, one should also give direct experimental justification for these hypotheses. Modern theoretical physics has cast this attitude to the winds.

The climax of mechanics as a physical theory came with Newton and in his Principia we see again the same feeling which actuated Galileo. In his enunciation of the "laws" of motion which Newton treated as "axioms" he felt the desirability of justifying them by statements referring to simple phenomena which anyone can observe without much trouble. We now feel that Newton rather exaggerated the obviousness of the fundamental principles and that in particular the concepts of mass and force are inadequately defined. However, the main point is that the postulates as he gave them were still closely tied to experience. Nevertheless, and this is of the utmost importance for our present discussion, Newton visualized a program in which these postulates of mechanics which seem so evident from experience

(even if unprovable directly thereby) should be used as a basis for the description of all sorts of physical phenomena. In other words he gave the first impetus to the dynamical theories of physical phenomena which were so successful in the 18th and 19th centuries.

Stimulated by Newton's marvelous success together with that of his successors on the continent, including Euler, the Bernoullis, Lagrange and Laplace, there grew up the feeling that in the fundamental concepts of mechanics mankind had a veritable Novum Organum for the description and understanding of all natural phenomena. Gradually the earlier method of choosing concepts and postulates closely related to the observed phenomena, the method indeed which had led to the very successful development of mechanics itself, was abandoned in favor of the search for mechanical theories of everything. The triumph of this program during the 19th century quite overshadowed any question as to the subtle way in which the hypotheses of the theories were drawing further and further away from experience and allowing more and more scope for the inventive imagination of the researcher. A good illustration is provided by the kinetic theory of gases in which the application of mechanics to aggregates of hypothetical (invisible) particles yielded results in astonishing agreement with some of the observed properties of gases (e.g., the derivation of Boyle's law, etc.). Another illustration is given by the increasing use of the purely mechanical concept of energy during the 19th century in the study of thermal and electrical phenomena.

It must not be thought, however, that this method of physical theorizing went completely unchallenged. In Faraday, the greatest experimental physicist of the 19th century, we meet a mind more nearly akin to Galileo's in its insistence that the framing of a theory for a particular set of experiences should proceed through the choice of hypotheses suggested as directly as possible by the facts themselves. Departing entirely from the atomic point of view (a direct heritage of Newtonian mechanics) and the action-at-a-distance theory (a direct descendant of the Newtonian theory of gravitation) Faraday preferred to create

an entirely new concept—the line of force and a medium for its location. By virtue of long association he came to think of these lines of force as the natural way to interpret his experiments. We must admit that they proved very successful in suggesting new experiments and results. To Faraday they doubtless seemed more real and plausible than the centers of force of classical particle mechanics. It is doubtful that he felt very keenly the desirability of interpreting everything along mechanical lines. Maxwell was greatly influenced by Faraday's ideas and without question this influence led to the electromagnetic theory of light, which Lord Kelvin claimed he never could understand. The history of physics during the latter part of the 19th century and the beginning of the 20th might seem to indicate that Faraday was even wiser than he knew in his willingness to adopt new concepts, intimately connected with experience. For the attempted application of classical mechanics to the kind of experience which we now call atomic phenomena broke down badly. It is one thing to agree with Poincaré that a mechanical theory is possible for every physical phenomenon, but it may well be too complicated for our feeble intellects to play with and therefore of little use for physical description. It is consequently not surprising that the development of modern physics has witnessed the increasing tendency to invoke hypotheses which spring not directly from experiment but rather from the imagination of the theory framer and whose plausibility becomes marked, if ever, only by long study and contemplation. This is certainly the situation in quantum mechanics with its ψ function, its curious operational analysis, its *possible* observable values, etc. But the present status of physical theorizing is even queerer and more complicated than our previous brief historical survey may indicate and we must pause to examine it in more detail.

In the early development of physics the macroscopic character of experience inevitably suggested description in terms of the concepts and hypotheses of mechanics. This was particularly true where the actual motions of large scale bodies were concerned. As physical experimentation became more elaborate and further removed from purely mechanical operations description

in terms of mechanical concepts demanded increased ingenuity. Strictly speaking therefore if physical science had followed the lead set by Galileo it should presumably have expected to make the greatest progress by devising new concepts and postulates suggested to the imagination directly by the new experiments themselves. This appears to have been Faraday's idea. However by that time the theory of mechanics had become so ingrained into the thinking apparatus of physicists that many if not most felt they could not describe things satisfactorily without it. This feeling was natural when we consider the tremendous previous success of mechanics and the fact that practically all physical measurements involve mechanical characteristics to a greater or smaller extent. The essential difficulty with this attitude became most marked with the advent of atomic physics and the breakdown of classical mechanics.

In attacking the problem of describing atomic phenomena two courses were possible: 1) to continue to use the fundamental concepts of classical mechanics, viz. concepts of particle, displacement, velocity, acceleration, energy, etc., but to handle them by means of hypotheses abstract in character and suggested directly by nothing in the phenomena themselves or; 2) to break away from the concepts of mechanics entirely and build up a new set on the basis of the actual experiments. Quantum mechanics is the result of choosing the first alternative. We may well ponder the significance of this choice. There are advantages and disadvantages to both. The first has the merit of continuing to utilize the fundamental ideas with which physicists have long been familiar. To that extent it satisfies the desire for the *unity* of scientific theories—the urge for a single fundamental point of view in the description of all physical phenomena. It represents the feeling that our experience can never become so complicated as to render impossible the use of concepts constructed to describe more common or more readily obtainable experience. However, there is no guarantee that this feeling may not be a mere illusion and herein lies one of the dangers of the choice in question. By sticking too close to the older constructs we may be unduly cramping our style and actually preventing progress in under-

standing. Certainly in following quantum mechanics as a model theory we must expect the way in which the mechanical concepts are used to be very different from that in which they appear in classical mechanics. This difference is appearing in nuclear theory in even more exaggerated form from that assumed in what is now somewhat euphemistically termed "classical" quantum mechanics, i.e., that which applies to the ordinary extra-nuclear atomic properties. If we are to have increasing abstraction in any case, some might well ask why we do not go the whole way and build a completely new structure with some nice new concepts very closely associated with the phenomena being described. It is very interesting to note that when Heisenberg first suggested the procedure we now know as quantum mechanics he adopted what is very close to this point of view in his emphasis that quantum mechanics should deal only with actual experimental observables. Curiously enough a few years later he got cold feet because people complained that this point of view was not sufficiently "anschaulich," and the result was the famous 1927 paper which introduced the indeterminacy principle and other things about quantities that are distinctly not observable from the practical experimental point of view. Probably he felt unwilling after all to cut adrift from mechanics entirely: it would mean placing a bridgeless chasm between classical physics and quantum physics. As it is, we have kept a bridge even if it is one that few people care to try to cross.

It is to be feared that the above description of the situation of modern physics with respect to the future theoretical development may appear in the guise of an unhappy dilemma on the horns of which we, whose lot it is to study natural phenomena, would seem to be cruelly impaled. On the one hand the attempt to build an atomic theory using concepts directly derived from the experiments themselves, while it gains in plausibility and makes a more vivid appeal to the experimentalist who naturally thinks most clearly in terms of his experiments, also sacrifices the unity of physics by its abandonment of the concepts which have stood so well the test of time in physical theories of macroscopic phenomena. On the other hand, in order to build an

atomic theory using the concepts of previous experience we are forced to abandon the hypotheses of classical physics and introduce other notions (e.g. the ψ function and the Schrödinger equation) whose abstractness is witnessed by their purely analytical character and for which logical analogies are not to be found in the macroscopic physical world.

What attitude should we take toward this dilemma? A good deal depends on our philosophical outlook on the fundamental nature of physics. If we think of physics as really the discovery of an external universe wholly independent of ourselves, it would seem most reasonable in carrying out the discovering process that we should not assume more than the actual experiments and measurements warrant. Presumably we might well follow Francis Bacon in his famous dictum:

"Our method is to dwell among things soberly, without abstracting or setting the mind farther from them than makes their images meet, and the capital precept of the whole undertaking is that the eye of the mind be never taken off from things themselves, but receive their images as they truly are and God forbid that we should offer the dreams of fancy for a model of the world."

The trouble with this is that the world of modern experimental measurements is immensely more extensive and complicated than Bacon ever imagined. Moreover if we take the point of view that physics is really the *description* of our experiences without any reference to a hypothetical external world, it seems that we ought to preserve our liberty to choose whatever concepts and hypotheses are useful to us no matter how abstract or far removed they are from experience. Our guides to new theoretical constructions would then be the following: 1) to use all our previous theories to the limit before the invention of essentially new points of view; 2) in introducing new hypotheses, to choose such as can be most easily and simply manipulated mathematically and in which the identification with experience in order to produce verifiable laws follows with a minimum of artifice; 3) to choose such constructs as will permit a maximum number of deductions and predictions of new phenomena. All items in this directory are important but none more so than the last, for it is precisely this

element which controls the interest of experimental physicists in the theory and the attention they pay to it. This provides, so to speak, the social aspect of a theory, an aspect probably not sufficiently recognized in the past. A theory is of little use unless competent people give heed to it, no matter how meritorious it may seem in the abstract.

It is difficult to resist the feeling that the development of physics is proceeding along a normal line and one that may be expected to lead to new successes. At the same time we must be prepared for the possible advent of a new Galileo, Newton or Faraday with a brand new synthesis. Such things are in the lap of the gods! In any case, inevitably someone rises with the embarrassing question: of what use is modern theoretical physics if it can be understood by only a few? Should the experimental physicist be content merely with a collection of formulas which the theorist gives him to verify? The only reasonable answer to this is an emphatic negative! But the problem of understanding physics is after all largely one of pedagogy. After some brilliant intellect has broken through a path where the way seemed impassable, it is up to another group to make the path smooth and remove the roughnesses characteristic of every new theory. These people must take the trouble to understand the theory in the form proposed by the authors and then "translate" it by liberal use of analogy into a form which can be understood by those to whom its use is vital. This incidentally is wholly apart from the question of the popularization for the general public which demands quite another sort of technique. In this connection we note the interesting fact that it is not often that the author of a theory can also serve as the translator. Illustrations readily come to mind so that no names need be mentioned. No discredit should attach to the brilliant discoverer for failure to make the translation. It requires quite a different technique from that of discovery.

It should also be borne in mind that new ideas require time to sink in. In time they become familiar and hence simple. Just as the mechanical notions of Galileo, which appeared frightfully abstract in his day, are commonplaces today, so the ψ function

and the other mysteries of quantum mechanics will become common-place tomorrow.

One of the things we ought to learn from such a discussion as we have been trying to give is that we can expect no unanimity in answer to the question: What method provides the surest hope of success in future physical research? We cannot even hope for agreement as to the essential meaning of physical theorizing itself. Much of this business is a matter of taste which some take seriously and others not. A clear illustration of this fact occurred recently in the columns of "Nature."[1] It began with a blast by Prof. H. Dingle against what he terms the modern Aristotelians (perhaps some would prefer to use the term neo-Platonists), i.e., those who think that physics can be got out of one's head without recourse to experience. He compares this group unfavorably with the followers of Galileo who still consider experiments and measurements fundamental. Among the "criminals" Eddington and Milne appeared rather prominently. Naturally there was a reply to this "attack" which brought out a refreshing though somewhat disconcerting variety of opinions. Space does not here allow a complete analysis of these, but we ought at any rate to note the rather sharp cleavage between two schools of thought. Eddington has reached the conclusion best stated in the form he has given it in his book "The Relativity Theory of Protons and Electrons" (p. 327): "there is nothing in the whole system of physical laws which cannot be deduced unambiguously from purely epistemological considerations. An intelligence, unacquainted with our universe but acquainted with the system of thought by which the human mind interprets to itself the content of its sensory experience should be able to attain all the knowledge of physics that we have attained by experiment." This is an interesting statement. Taken at its face value it would seem to express the opinion of its celebrated author that the experimentalist might as well leave his laboratory, lock the door and hang out a sign: "Closed until further notice; inquire at the Library." If it is indeed true that we learn all physics

[1] *Nature*, **139**, 784, 997, 1025, 1937.

ultimately out of our heads, then we are certainly going to unnecessary expense to maintain elaborate experimental laboratories. A sober second thought, however, convinces us that not even Eddington believes all this in the literal sense. All he is trying to express is that there is something about the nature of our minds which makes us describe our experiences in the way we do. Actually we must never forget that the *physicist* makes the laws in the physical world. *He* creates the theories; it is *his* description of Nature which we have been talking about all along. From the view of the nature of physical theory presented in this paper it is of course *possible* that one who had never heard of a physical experiment could by building certain concepts and relating them with certain postulates deduce all physical laws. There is certainly nothing impossible about this. The only catch about it is simply that it has never been done, not even by Eddington. Moreover, if physics *is* really a description of experience and not some mere trick game played by the mind with a lot of symbols for its own pleasure, it never will be done, since we cannot begin to describe experience until we have had it. Though we hope our theories which describe past and present experience will also predict correctly that of the future, that hope remains a merely pious one until experience verifies it. Physics should continue to leave to the mathematician the fascinating game of playing with symbols, while remaining perfectly willing to utilize all the mathematicians' subtle dodges in the construction of new physical theories.

What then of the future of physics? Do we know it all or don't we? I think we all agree that we don't, even if Eddington seems to have some doubts about that. How are we going to describe our future discoveries? No one can be sure but it is a rather safe guess that we shall for some time continue to construct theories by the method which has proved successful in the past, i.e., by utilizing to the limit all our time-honored constructions but not hesitating to invoke our imaginative powers in the endeavor to make them fit new situations. The *distant* future is a closed book no matter how fascinating speculation about it may be. One can only have feelings about it. The author cannot

resist holding the attitude that we are but children in this business and that our descendants a millenium or two hence (if there are any left then) will consider our physical ideas, if mayhap any record of them survives, the very acme of puerility. Probably they will wonder how ancestors of theirs could ever have been so lacking in vision as to fail to see the really essential elements in experience. However, we need not feel unduly humble about this, for those who come after us will also be forever deluding themselves as we have done, their strivings like ours will be rewarded only by transient successes; they too will come to the conclusion that all mankind can do in organizing knowledge is to make mistakes; they however will share the hope that rises in the breasts of all of us that we can still learn enough from our mistakes so as not to make the same ones over and over again to all eternity.

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Telepathy, Psychical Research, and Modern Psychology

BY

H. ROGOSIN

The widespread publicity and the consequent furor created by Dr. J. B. Rhine's experiments on telepathy and clairvoyance, have been interpreted in some quarters as a call for a thorough-going revision of the entire field of psychological thought. This revision, it is held, must be in the direction of philosophic idealism, and away from previously accepted materialistic doctrines. The present paper points out the non-verifiable nature of the entities postulated by the believers in "supernormal" phenomena. Furthermore, the interpretations of contemporary science held by such individuals are not in themselves on a scientific plane. Specific errors in reasoning made by workers in the field of psychical research, are presented. In short, "psychical research. . . cannot turn the universe into a non-material, non-objective one, on either the basis of misinterpretations of current physical thought or on the basis of its own work."

PSYCHICAL Research, as the study of the supernormal or the supernatural, has been frowned upon for many years; the very thought of it, in fact, has been anathema to a great number of scientists. But the attitude of both laboratory workers and laymen toward this field has been changing in the last few years. In the first place the works of latter-day physicists such as Einstein, Planck, Schrödinger, and de Broglie climax an era in which our conceptions of the universe have altered. The newer physics eliminates absolute space and time and this has been taken to mean that "Time and Space are constructs of the mind" (16). Now that physicists have changed their former conceptions of

the universe, "it becomes necessary to change our conceptions of life, mind and man" in the light of these changed ideas (16). In the second place, the work of Drs. J. B. Rhine (22, 23) and J. F. Thomas (31) at Duke University, seemingly has placed supernormal phenomena upon a scientific plane of experimentation and verification.

These two developments point to the crying need for an overhauling of psychological systems of thought.

At least so runs the argument. But is it a valid one?

Modern physical thought can only be evaluated with the following kept clearly in view:

1. The dichotomy existing between the actual work of scientists and their theorizing about that work. For example, the metaphysical speculations of men like Jeans and Eddington are on a different level of reliability from their actual laboratory procedures. The reader must be careful to distinguish between the literary endeavors of scientists and their more analytical, descriptive experimentation. Especially is this true when inferences are made to extend outside the particular sphere of competency of the writer. In any case, decisions on questions raised, must be made on the merits of the facts. Those individuals who appeal to authority as the final arbiter of judgment, are clearly out of order.

2. Eminent men, including Nobel Prize winners, can and do make errors in reasoning which are discoverable by lesser men.

With the above as a basis for understanding, we can proceed with our analysis. What is meant by that all-important sentence: "Time and Space are constructs of the mind"? There can be no other meaning attached to it than the idealistic one that they do not exist of and by themselves; that they exist only as thoughts. Yet that is patently false and needs no belaboring. Time and Space existed before mankind arrived on the scene, and will exist even if mankind is wiped off the face of the globe. Science has not yet come to the conclusion that only your thoughts and mine exist in a world that is itself only a thought.

Einstein, Max Planck, and many others among the atomic and relativity researchers are believed to have said:

1. Space and Time are relative.
2. All is relative.
3. Therefore everything exists only in mankind's "mind".

This is a misconception. The theories of relativity and atomic physics can only be interpreted to mean that:

1. Space and Time are relative. That is, the physical environment is relative to perception of changes in that environment.
2. All is relative, *in an objective world.*
3. The sequences of events represented by the concepts Space and Time, exist independently of any individual's thinking processes.

The main point is that this universe is an objective, material one. The contention that physicists have introduced valid conceptions of non-material entities; have determined *experimentally* that the universe is nothing but a figment of the "mind"; have introduced in other words, the theory of idealism as a *scientific* theory, is not warranted by the evidence. Recent developments in physics have not justified the introduction of new categories, which differ in *kind* from the categories used during preceding periods.

It is important to remember that the term material is not a static one but is dynamic, and changes its meaning as the course of science itself changes. Today it means much more, for example, than it did in the 19th century when matter was considered as different from energy. Since that time a synthesis has been made showing the essential similarity and relatedness of these two supposedly different entities. But even in the 19th century and before, material in the philosophical sense, could have and has been regarded as opposed to immaterial, the empirical as opposed to non-empirical. Materialism as a school of thinking, must not be used as it is all too often in the popular magazines and among laymen, as standing for crude greed or as by definition excluding those values of life which are classed as the "higher" ones, i.e., the appreciation of poetry, the emotional factors of personality, the pleasurable reactions of mankind, etc.

A further point: in the history of science, which is coextensive with the history of civilization, the immaterial, the non-empirical does not exist. Whatever is scientific, is to be considered as true by definition: Truth and Science are equivalent. These two terms also are not absolutes but relative in nature and shift in accordance as our stock of information changes. By no means is it to be considered as some philosophers have had it, that the use of a term in logic implies the existence in the material universe of whatever it is that has been handled verbally. To say that an immaterial entity exists is to give voice to a contradiction caused by the failure to get behind "The Tyranny of Words".

Therefore a new deal in psychology is not needed on the basis of what has happened in physical thought.

But the special plea for the inclusion of psychical research in psychology must also be judged on its merits alone. Do "extrasensory perception" and "supernormal cognition" exist? It will be sufficient here to point out some of the assumptions made by Drs. Rhine and Thomas and analyze them. There is no doubt whatsoever as the authors themselves have recognized, that their experimentation is revolutionary, so revolutionary that if upheld, the whole systematic developments of Science up to the present day would be overturned.

Before setting out upon the monumental task of changing the whole systematic development of Science, however, it is necessary to determine if the evidence advanced in support of such far-reaching claims as have been made, is reliable and valid.

Any purported evidence which discards theories that have a long history of sound experimentation behind them, must be gone over with a fine tooth comb; must be scrutinized with the greatest of care and attention; and must not be accepted unless it is beyond all cavil. After all scientific method means that starting with certain assumptions, working hypotheses are derived. These hypotheses are either rejected or proven valid by experimental means. In all experimentation there must be adequate controls, soundness of work, careful attention to details; and the most exacting safeguards must be taken to eliminate all other hypotheses before accepting the one under investigation.

Neither Dr. Thomas's nor Dr. Rhine's experiments have been so adequately controlled as to eliminate all other hypotheses except the one of supernormal or supernatural causation. Dr. Thomas's evidence supposedly indicated a 92.3% probability of the survival of the "spirit" after death! Some kind of non-material entity was postulated which in 525 sittings with mediums communicated through them and gave information. It is essential to remember that this work was done at Duke University, and under the direction of Professor William McDougall who supervised as head of the department of psychology, the work of Dr. J. B. Rhine. Both the "extra-sensory perception" experiments and the "supernormal cognition" ones were based upon interpretations of probability theory which will be analyzed later on in this paper.

In connection with the Rhine work, it is interesting to point out that one of the most severe criticisms that I have read is to be found in an article by Prof. R. H. Thouless, published in the London "Proceedings of Society for Psychical Research", April, 1935. It is there stated that "This unfortunate concentration on fantastic anti-chance probabilities seems to have led him into paying quite insufficient attention to reporting the precautions taken against other sources of error" and that "it is generally quite impossible to discover for any particular experiment what the experimental conditions were".

Nevertheless, far-reaching conclusions are put forth. There is the definite upholding of a "de-materialization of mind" and the hypothesis is raised that "a new mode of energy reception is required. . .reception of an unknown energy form by an unknown mode of reception" (22). In some peculiar way this energy form affects the nervous system, as shown by the different drug experiences of subjects, yet it is non-material in nature!

Dr. Rhine overlooks possibilities more in accordance with scientific principles and thereby does not realize that he is flouting scientific maxims such as the one that non-material objects do not exist. The whole system of cause and effect by which science has progressed is built up from this base. Our whole body of knowledge has been gained through either its implicit or explicit use. Granted that natural laws are only

approximations, only "statements of statistical averages", nevertheless those statistical averages are based on the known composition of atoms and their behavior. Causation has not been eliminated simply because we have been forced to revise our physics and at present talk in terms of mathematics (3).

Science continually changes its conceptions, and as many philosophers and scientists have pointed out, is a self-corrective system of thought. The simplest explanation for events is the one accepted as valid until overthrown (Occam's Razor). That is, simplest in view of the existing evidence, and the relationships within the larger framework of science itself. Now the hypothesis of supernormal or supernatural causation, is not the simplest explanation: it involves a denial of other fundamental hypotheses such as the one called "The Hypothesis of the Sensorial Gateway" (John E. Coover) which means that information is garnered through the senses. Furthermore, nothing more than the bare existence of telepathy and clairvoyance is adduced by Dr. Rhine. What these things *are* is not explained, nor is any relationship with other phenomena shown. It would be more in accordance with scientific methodology to say that an improbable explanation of unusually high scores in guessing card faces, based on normal means of communication, is far superior to an explanation which postulates a supernatural means of cognition. The idea of such a means of communication comes into immediate conflict with sound experimental work on psychological subjects such as thinking, learning, hypnotism, abnormal and so-called supernormal events. Such a hypothesis raises a great many more questions than it answers. The proof of the pudding with regard to any new idea is that it answers more questions than any other available theory, and answers them more adequately. This cannot be claimed for the hypothesis of telepathy and clairvoyance as an explanation for correct guesses of cards.

As for the statistical proof for telepathy and clairvoyance, and "supernormal cognition", Davis and Nelson have said in their "Elements of Statistics":

"Statistical methodology is no magical or even mechanical, instrument that automatically grinds out valid conclusions and

allows the suspension or avoidance of personal judgment. Indeed, it may be said flatly that a statistical conclusion is no better than the judgment of the statistician who produced it." (p. 339), and "The literature of statistics is replete with examples of the formal application of techniques where the interpretation and significance of the results is inaccurately, if not erroneously, stated. Only a fundamental knowledge of the assumptions which underlie the mathematical formulas can protect the worker against the pitfalls ever present in the investigation of statistical data" (p. ix.).

E. G. Boring says too that the case is not uncommon "in science in which inadequate data are treated with elaborate mathematics, the precision of which creates the illusion that the original data are as exact as the method of treatment" (1, p. 249).

Moreover, the laws of probability of which the proof for both "extra-sensory perception" and "supernormal cognition" depends, cannot be applied indiscriminately. They imply, as pointed out by John Maynard Keynes in his "Treatise on Probability", a relationship to a body of knowledge. All available evidence is used to determine the initial probability of the event being investigated, and also is brought to bear on the problem. For example, if a perfectly smooth ball is to be rolled down a perfectly smooth incline, we can, given these conditions and other pertinent information, determine fairly accurately, the velocity of the ball and the time it will consume in rolling down the incline. We can predict with a fair degree of success. But given an irregular ball, and an irregularly distorted incline, it is extremely difficult if not impossible to determine the velocity and time factors. It may be impossible to determine accurately even the conditions themselves under which we are working!

A thorough knowledge of all information bearing on a problem is necessary in determining probabilities. If we neglect any background material, our calculations themselves may be correct, but they lose their value as predictions or as determinations of the probability involved in the particular problem under investigation.

Most important too, is the fact that the theory of probability

cannot prove the existence of something not demonstrated to exist through other means. This applies to the whole field of mathematics as well. And so far the existence of any extra-sensory means of gaining knowledge, or of "spirits" has not been proven by Science. Behind probability theory are the more fundamental assumptions of Science itself, its methodology and its accumulated information.

So it has been claimed that both Dr. Rhine's and Dr. Thomas's work has been invalid because of a lack of initial probability for their hypotheses. In the second place, because of the complex nature of the telepathic and clairvoyant experiments, given the Duke University conditions, it is all but impossible to determine all the factors affecting scores in guessing cards. What are the chances of guessing card faces correctly in a pack of twenty-five, when conditions are varied irregularly? (Compare the ball illustration above.) That is essentially the problem. Under the laboratory procedures in use at the southern university, not only are the chances difficult to determine (the matching technique that is required is rather complicated), but they certainly have not yet been determined by the investigators.

As for the enormously long odds against chance found by experimenters on supernatural events, Professor R. A. Fisher, eminent mathematician of the University of London, had the following to say in a personal letter to the author of this article:

"...I have, before now, criticised their cogency on grounds, not only that the procedure of calculation is often questionable, but that they are much less relevant to the establishment of the facts of nature than would be a demonstration of the reliable reproducibility of the phenomena."

There are two other points bearing on this same question of long odds against chance. In the first place, what is meant by the term chance? If it stands for "accidental" factors, what can they be that give such huge figures all in one direction? Does that not indicate that something is wrong somewhere in the experimental set-up? That possibly these "accidental" factors have not all been cancelled out as postulated by theory and have become a part of the procedure to such an extent as

to give positive results. Under such conditions they cannot be considered "accidental" in any true sense of the word, and the term "chance" is unjustified. Secondly, if it stands for uncaused events, it is patently absurd and ruled out through the experimental history of Science. It can be upheld logically that in our material universe, there is no such thing as "chance", and that the word is important only because it has been used so continuously in the past as to prevent its dying out after outliving its usefulness. Things don't happen *because of "chance"*; they happen because of certain other events or happenings. Causal sequences are involved at all times: Space-Time sequences. Therefore in the view of this writer, the continual talk of long odds against "chance" achieved by the investigators of supernatural and supernormal events has had an obscuring effect. It has prevented people from focusing their attention on the main issue: what specific factors are responsible for the results found. The reasoning has been: it can't be "chance", therefore it must be ESP ("extra-sensory perception"). This jump from "chance" to one of many alternative hypotheses, is entirely unjustified and definitely illogical.

From the experimental evidence which, as pointed out above, is not adequate or reliable, Dr. Rhine goes on to draw implications which would not be justifiable even if the evidence were adequate and reliable. Under the aegis of Science, he introduces a belief in spiritism. At the New York *Times* 1937 Book Fair he spoke as follows:

"If these experiments in extra-sensory perception lead thus to a de-mechanization of at least certain processes of mind, they are at the same time proving some ground for the older and much-discarded spiritualistic philosophies of mind which would have given mind a place in the universe in its own right, an ultimate reality of its own, with a favoring of at least the possibility of an unbroken survival, regardless of the outcome of its bodily associations. . .the detachment of mind from the accidents of space and time."

These words of Dr. Rhine are startling to say the least, and their meaning is perfectly clear. The writer does not believe

that any charge of gerrymandering citations (see 24), can refute the fact that words do have meanings attached to them, and that the terms used above can be interpreted in only one way: that there is a definite possibility arising from the experiments on "extra-sensory perception" of the "mind's" existing by itself *without* any connection with the body, and also *apart* from time and space. There are three viewpoints possible here: 1. Dr. Rhine does not mean what he said; 2. he has erred in the implications he has drawn from the Duke University work (entirely aside from the question of its evidential value); 3. he is essentially correct in his attitude.

It is impossible to go into the problem of what it is that was meant except through the objective evidence in the statements made. Turning to the quotation given then, it must be said that there can be no such entity as a "disembodied mind" or "spirit" which does not have any causal relationships in Time or Space. Nor as was pointed out in the discussion of new physical theory, is it scientifically admissible to talk in such terms as those quoted above. Therefore the stand of this writer is in line with the second viewpoint.

Psychical research then, cannot turn the universe into a non-material, non-objective one either on the basis of misinterpretations of current physical thought nor on the basis of its own work.

In the above essay only the more important assumptions involved in the question of psychical research revitalizing or revamping psychology, have been touched upon. Certainly it would seem from the history of Science that a form of communication going on between "mind" and "mind" apart from the recognized channels of sensation (telepathy), and related forms of communication, cannot be admitted as true phenomena.

A new deal in psychology is not needed. As Joseph Jastrow pointed out as far back as 1900: "It is not the suggestion of inadequacy (i.e. of psychology) that is significant, but the concrete facts and evidence available to prove (its) inadequacy". Furthermore, no scientists and no science will protest if an accepted doctrine is displaced for a new, more adequate one.

The doctrines of psychical research are not such however, as to warrant the displacement of our older psychological principles. When the future historian of Science sits down to characterize the present wide-spread belief in "extra-sensory perception" and "supernormal cognition" he will undoubtedly classify it as the 20th century counterpart of phrenology: that quaint belief of the 19th century that you could tell what was in a man's head by the bumps upon it.

To summarize: A revision of psychological thought in the direction of the anti-materialistic doctrine of philosophic idealism, in the direction of the Platonic belief that this world is but a shadow of an "Underlying Reality" of some kind or other, is not based upon anything but wish-fulfillment. Scientific progress in the physical sciences does not support the argument for a fundamental change of direction. The experiments at Duke University and elsewhere, upon "extra-sensory perception" and "supernormal cognition" are based upon faulty assumptions regarding probability theory, the nature of proof, the place of mathematics in the development of Science; in short, faulty assumptions regarding philosophy of science. Moreover, experimental procedures employed, as a totality, cannot be said to be so adequately controlled as to eliminate all other factors except the one experimented with.

Neither internal nor external evidence can support the idea that a means of gaining knowledge without the use of the senses, has been achieved.

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Encyclopaedism as a Pedagogical Aim: A Danish Approach

BY

OTTO NEURATH

I

"ENCYCLOPAEDISM" is the systematically pursued activity within the Unity of Science Movement, devoted to the collecting of scientific statements of the logic of science (synonymous with "Philosophy of Science" for Carnap and other representatives of modern Logical Empiricism) logic, mathematics, physics, biology and other sciences, including all the "cross connections" between the given disciplines. (See: Otto Neurath, Unified Science as Encyclopedic Integration; in: International Encyclopedia of Unified Science, Vol. I, No. I.)

Unified Science found on scientific statements removes all "isolated sentences", a term proposed by Reach for formulations which cannot be used for the sciences. Is such a pure scientific Encyclopaedism in a position to satisfy human yearnings and to create "oceanic feeling"—if we may use Freud's term in this case? This question will be answered by the Man of the Future. But we know some young people who like to rummage in all the

disciplines for stimulating revelations and to thumb over old and new books impelled by an enigmatic desire to embrace the whole of human knowledge. But how rarely do scientific teachers stimulate and gratify this passion!

Joergen Joergensen, the well-known author of "A Treatise of Formal Logic" (a general survey of the development of modern logic) and one of the organizers of the *International Encyclopedia of Unified Science* (Univ. of Chicago Press, Monographs 1 and 2, 1938) lectures to students of all departments at the University of Copenhagen, as an introduction to scientific argumentation in general. Since these lectures are "printed as manuscript" I am in a position to report briefly on this case of *actual "Encyclopaedism" within the sphere of a university.* I only want to outline this educational work done by Joergensen so as to provide a picture of the *methodological problems* but it is not my task to analyse Joergensen's special empirical views. I add a few *historical remarks* which may help to characterize the intellectual environment in Denmark which is, in certain sense, more similar to the intellectual environment in the United States, England, France, in the former Austria, or Poland than in Germany.

It is necessary to collect material for a comprehensive history of Logical Empiricism which would be a stimulating educational aid to the preparation of general encyclopaedistic scientific courses. (For a few remarks touching on this problem, see "Le developpement du Cercle de Vienne et l'avenir de l'Empirisme Logique" par Otto Neurath. Paris: Hermann & Cie., 1935). The usual books dealing with history of philosophy do not reserve much space for the *history of scientific empiricism.* Such a history must be a special subject for books written by men of the Unity of Science movement. (See Actes Du Congrès International De Philosophie Scientifique, Sorbonne, Paris, 1935, Paris: Hermann & Cie., 1936, Pamphlet I: Charles W. Morris, Semiotic and Scientific Empiricism and Pamphlet VIII: Joergen Joergensen, The Development of Empiricism in Scandinavia).

The name "University" is not only derived from the Latin "universitas literarum", but we find continental "Philosophische Fakultäten" which show the relics in our time of the traditional

encyclopaedical ideal of the scholastic period. "Fakultät" in Europe does not mean the same as "faculty" in the United States; "philosophische Fakultät" is a gigantic department of departments, and a student can attend lectures of very different kinds *within his own sphere*, which is determined by the choice of the "Fakultät" only; such lectures may deal with pure philosophy, linguistics, literature, geography, history and social sciences, history of arts, theory of music, psychology, botany, zoology, astronomy, physics, chemistry, pharmacy, mathematics, logic, or other sciences. The flavor of comprehensiveness is a characteristic of such "Fakultäten" and it is not unusual that a student concerned with a special discipline frequents lectures by a professor of the same "Fakultät" who teaches a very different subject *throughout his entire academic career.*

In the United States each single department offering *graduate work* is as a rule destined for a more or less specialized theoretical or practical aim; therefore if a logician gives lectures as a member of a department of philosophy he has in general a different contact with the students than if he were to give lectures in logic as a member of a department of mathematics. In a continental university of the old style it is assumed that lessons in logic are given for all students of the "Philosophischen Fakultät", that means for historians in the same way as for mathematicians or pure philosophers.

It was an innovation in some continental universities when the "Philosophische Fakultät", overcrowded with disciplines, was bifurcated and a special "Fakultät" for Mathematics and Natural Sciences was founded, as in Copenhagen for instance. But the old tradition of "universitas literarum" remains in many cases, and we find attempts made by various universities to collect students of different disciplines for certain philosophical lectures mainly devoted to metaphysical instruction.

II

Every student of the University of Copenhagen is obliged to attend a special series of lectures in philosophy as part of his course of study, whatever may be the department in which he does his major

work. These courses are offered by different men having different points of view and emphasizing different things. Professor Joergensen has given such courses since 1926–27. His main task is to lead students of all kinds to understand scientific thinking in general. He does not give a general introduction to the logic of science with certain applications, as do for instance Morris R. Cohen and Ernest Nagel in *An Introduction to Logic and Scientific Method*. Joergensen's students learn the "grammar of science" by seeing in what way the leading scientists found their fundamental ideas and why changes were unavoidable. The proportion of space occupied by the chapters in his syllabus is very significant: the main part of about 450 pages deals with mathematics and logic, physics, biology, psychology, history and social science,—including science of religion and linguistics. The second part of about 100 pages explains problems of European philosophy, or more exactly formulated, problems of English, French, German, and Scandinavian philosophers. This division of the text has some very instructive consequences. Thus Leonardo Da Vinci and Galileo appear in the first part of the lectures as actual representatives of modern science who propagated the new ideal by their own scientific work; and while Francis Bacon, who only acted as a herald of empiricism but not as an investigator, appears in the second part. Joergensen considers thinking of primitive periods and shows the passage from anthropomorphism to modern science all through his lectures, especially in the sociological chapters. He explains in many cases in detail how to avoid scientific mistakes and difficulties bred by mysticism of various kinds. In the first lessons he comes quickly to scientific problems themselves. The short history of Greek philosophy for instance gives him an opportunity to explain in greater detail the most important mathematical discoveries of this age—this is as it were an introductory chapter on mathematics; in this connection you learn about the actual mathematical foundations and consequences of the Pythagorean doctrine of the harmony of spheres but not about the Pythagorean eating no beans.

Joergensen's pedagogical method is more or less in concordance

with the historical evolution of sciences, and may be also influenced by Comte's theory of the stages of human thinking. Similarly he goes into the problems of modern physics, biology, psychology, and social sciences, *always combining historical explanation and logical reasoning*. We find throughout these lectures general remarks concerning details of the history of civilisation, for instance explanations about the "awe of the unknown", magical thinking, epilepsy as the basis of mystic activities, and stimulating excursions dealing with folklore and ethnography. Another kind of excursion deals with mathematical and logical demonstration. Joergensen comes from Diophantus step by step to Descartes and analytic geometry, and in this connection introduces the theory of functions. He discusses the trignometrical functions especially the sine curves which are well-known as an useful aid within the logical framework of the physical theories. It would be very interesting to test the results of these lectures.

Joergensen's interest in modern logic gives him a very strong basis for all his argumentation. Each student of Copenhagen University who attends his lectures comes to know for instance what Leibniz performed in the field of general logic, before he learns about his monadism and theodicy. He has to hear a lot about modern logistics. Frege, Peano, Russell and Whitehead are for him more than mere names. Joergensen comes from logic to the problems of scientific deduction as an introduction to the evolution of modern physics and from the modern atomic theory and quantum theory to statistical problems in general. He discusses the formulation of natural laws, interpolation and, very cautiously, the problems of induction.

Biology, psychology and sociology are deliberately explained in close connection with the different scientific publications without any special attempt to find a unification of the very different kinds of scientific expression in this field; this is significant of a more sceptical opinion that psychology and related disciplines are in a situation similar to that of physics before Galileo and chemistry before Lavoisier.

Joergensen's students have to learn considerable biological and psychological details as a basis for criticizing the doctrines

dealing with teleology, vitalism, and similar metaphysical theories. We find the same attitude of empiricism in the chapter concerning historical method.

It is certainly very useful for the students to learn the difference between "science of religion" and "religion" itself. We know the similar difficulties which arise when people do not make a sharp discrimination between "*morals*" and the "analysis of morals" as a complex of formulations presented mainly in connection with transcendent metaphysics.

The chapters dealing with science are more or less up to date. The second part of the lectures concerning philosophical problems only outlines the most important tendencies in the history of philosophy. Joergensen's empiricism, which always emphasizes the Unity of Science, developed from the English and French traditions. He is influenced by Spencer's scientific atmosphere and illustrates Criticism and Romanticism through German philosophers only, but Positivism through Comte, John Stuart Mill, Spencer and similar thinkers. Within this part of his lectures Joergensen discusses ethics, esthetics, and similar disciplines.

I think this gives a picture of Joergensen's work. It seems to me an important problem how we can systematically reduplicate such lectures in schools of various types and not only in universities. Such courses can be given by one teacher or by a group of teachers in co-operation. This is also a question of educational method if we define as method, all means whereby the average student can learn what previously was the result of special genius, of special personal history, and of special environment. I think that many people who are interested in the Unity of Science movement and Logical Empiricism will also be interested in *Encyclopaedism as the actual representation of the Unity of Science movement.* An increasing number of scientists have sympathy for all these activities. A great many of them actively participate by various scientific investigations which support the bridges from science to science, by the unification of our scientific language and by the construction of Unified Science. Not a few representatives of the Unity of Science movement are multi-directionally interested in the vast mass of scientific

activities in all the different fields, but only a small number of persons are well informed about certain disciplines and it is rare for one person to actively discuss problems of a great many sciences.

III

Joergensen's career, and a certain Danish tradition very different from the German tradition, created the basis for the unusual comprehensiveness of this empiricist. For a long time he was engaged in practical work connected with industrial organizations. One group of his publications deals with social problems of the present; another with logical investigations.

More than once we found in the history of Danish thinkers an important versatility and even a certain adventurous spirit combined with cautiously sober judgment and strict intellectual discipline. All foreign intellectual influences have been very strongly modified in Denmark. What a "mutation" of German Schellingism grew up in Denmark! The famous physicist Hans Christian Oersted is a very characteristic phenomenon. This many-sided man, influenced by Schelling, carried off as a young student in one year the prize for an aesthetic problem and in the ensuing year the prize for a medical problem, the title of his doctoral dissertation was "Architectonic Structure of Metaphysics of Nature." He had great success in different fields of science, literature and politics. Since there is only one university in Denmark every energetic thinker has an opportunity to exert extensive influence, and I should think that a touch of this comprehensive romantic scientist resounds in Joergensen's anti-romantic comprehensive Scientism.

Oersted's "*Considerations on the History of Chemistry*" are flesh of the flesh of modern Logical Empiricism. He explains for instance that certain elements which gave unity to scientific arguments are common to old fashioned mysticism and to modern science, and he demonstrates that Lavoisier's oxygen theory is not a rival to Stahl's phlogiston theory but its continuation, since both theories say that respiration is a kind of combustion, that we can put all bodies in a certain order according to their com-

bustibility, and that "reduction" is a chemical opposite to "combustion". Oersted admits that certain additions made to the "Symbol" X by the phlogiston people created trouble but that it is the logical structure of theories together with their empirical consequences which are most important. In this intellectual environment grew Joergensen's simple and hard protest against a great many persons, including leading scientists: "Hypostatization of theories may be a matter of thrilling physical novels—its value for understanding is nil".

Another factor of Joergensen's intellectual environment is Harald Hoeffding. His well-known comprehensive culture and capacious work prepared modern scientific empiricism in more than one direction. His antimetaphysical psychology influenced not only Danish students and scholars. At a time in which Windelband did not even mention the *name* of Ernst Mach in his short presentation of modern philosophy (see his paper in the series "Kultur der Gegenwart" B. G. Teubner, Leipzig), Harald Hoeffding analysed carefully Ernst Mach's work. This in a certain sense is characteristic of Hoeffding as it is characteristic of Philipp Frank and Moritz Schlick that they very soon noticed and propagated the importance of Einstein's ideas for our thinking in general while a great many philosophers and specialists discussed doubtfully or antagonistically the new theory of relativity. This shows a special genius in knowing how to discriminate significant personalities. Hoeffding quoted a statement of Ernst Mach which is well-known in the Unity of Science movement: *That it would be of great importance to find a standpoint which has not to be changed if we go from physics to psychology, since all sciences together will in the future have to form a whole.*

It was a great pleasure for members of the "Viennese Circle" to find that Joergensen and other Danish people of our time are, like Hoeffding, very familiar with Ernst Mach's work. Joergensen's lectures are full of Mach's spirit, but he treats also his fundamental scientific analyses which caused Mach as predecessor of Einstein to help change the whole scientific thinking of our period. And last but not least, Joergensen is in a great many respects influenced directly by Niels Bohr, who is very

intensely interested in the vivid growth of science as a whole and not only in the growth of physics. At the second International Congress for the Unity of Science held in Copenhagen, 1936, he discussed the possibility of a more cautious language which reveal more open doors in the future of science than does our present language. He emphasized as Joergensen does the importance of the Unity of Science. Bohr and his friends constitute an environment which continues an old Danish tradition of empiricism. (See: Niels Bohr, Analysis and Synthesis in Science; in: International Encyclopedia of Unified Science, Vol. I, No. I.)

Joergensen is a robust empiricist, but he knows also very well the power of ratiocination within the logical framework of theoretical constructions. We may call his attitude, according to Gregorius Itelson's terminology, "*Empirical Rationalism*" the counterpart to "Apriori Rationalism" which has been especially criticized by Louis Rougier. The term 'Empirical Rationalism' may be used synonymously with the term 'Logical Empiricism'. Joergensen emphasizes that all the complicated and most important scientific theorizing *starts* with the experience and language of our daily life, that we also have to *test* all the theoretical results of all the sciences by means of the same aids. Joergensen gives in his lectures not only a program of the Unity of Science but he also shows this Unity as an actuality.

The Hague, Holland.

Robert Hooke[1]

BY

G. J. WHITROW

'IT IS impossible for us to understand the character of Newton or the state of the physical sciences in the seventeenth century, without referring at length to the life and work of Robert Hooke.' So writes the author[2] of a recent biography of Newton. Well might we ask with him 'What manner of man was he whose personal opposition delayed the publication of Newton's *Optics* for thirty years and almost prevented the completion of the *Principia*; whose bitter tongue confirmed Newton's tendency to secrecy and isolation, destroyed his early enthusiasm for the Royal Society, and disgusted him with science?' It is not surprising that a man, capable of all this, should play a conspicuous rôle in the history of natural philosophy. Indeed a study of Hooke's career throws much light on the origins of modern science and, in particular, *it helps to determine the much disputed status of mathematics in the development of physical theory.*

Robert Hooke was born at Freshwater in the Isle of Wight on the 18th of July 1635. As a child he was extremely delicate and, in consequence, his father, who was vicar of Freshwater, 'laid aside all thoughts of breeding him a scholar.' Being thus left to himself he spent his time in constructing mechanical toys and at an early age he succeeded in making a wooden clock which worked.

[1] Based on the Commemoration Speech delivered in the Chapter House, Christ Church, Oxford on June 23rd, 1937.

[2] L. T. More, 'Isaac Newton' (1934) p. 92.

At the age of thirteen, on the death of his father, he went to London and for a short time was a pupil of the painter Lely, but 'the smell of the oil colours did not agree with his constitution, increasing his head-ache, to which he was ever much subject.' He then entered the School of Westminster and lived in the house of Dr. Busby, who boasted that sixteen of the living bishops had been birched with his 'little rod.' Under this famous Head Master Robert Hooke rapidly acquired Latin and Greek and a smattering of several oriental languages. What is more remarkable, especially in view of his later mathematical deficiencies, he is alleged to have mastered six books of Euclid in a week. Also 'he did of his own accord learn to play twenty lessons on the organ and invented thirty several ways of flying.' After such a remarkable school career it was natural that he should be sent to Oxford and, as an Old Westminster, to Christ Church. He entered the 'House' in 1653, as a servitor, and within two years his mechanical skill brought him to the notice of Oxford's leading natural philosophers, who recognized his inventive genius. In particular he became the friend and assistant of Robert Boyle and before long he exceeded his master in importance. He perfected an airpump, on the use of which Boyle's reputation so largely depends.

Among his manuscripts of this period we find the following typical passage. 'The same year I contriv'd and made many trials about the Art of Flying in the Air, and moving very swift on the Land and Water, of which I shew'd several designs to Dr. Wilkins then Warden of Wadham College and at the same time made a Module, which, by the help of Springs and Wings, rais'd and sustain'd itself in the Air; but finding by my own trials, and afterwards by calculation, that the muscles of a Man's body were not sufficient to do anything considerable of that kind, I apply'd my Mind to contrive a way to make artificial muscles; divers designs whereof I shew'd also at the same time to Dr. Wilkins, but was in many of my trials frustrated of my expectations.'

The seventeenth century was a period of enormous expansion of maritime trade. It therefore became more and more imperative that improvements should be made in the technique of

navigation. Consequently it is not surprising that while still at Oxford Hooke's restless and eagerly inventive mind should turn to this problem. With this question in view he devised many improvements in time-recording instruments and invented the balance spring for watches. However, he kept the details of all these discoveries to himself and we are therefore presented with the extraordinary spectacle of a young man, in his early twenties, claiming to have discovered a solution to one of the most urgent problems of his age—the accurate determination of longitude at sea—and yet refusing to publish his results. If he had published, it is probable that his plans would have brought him much wealth and an influential position. Why then was he so secretive? While we can discount some of his later claims because he was apt to confuse shrewd guesses with accomplished results, there is no doubt that at this period he deliberately concealed the results of detailed investigations. The reason for this extraordinary state of affairs must be that he realized that his ideas, although a definite advance, were not perfect and he probably feared that someone else would soon improve them and steal all the credit. This would certainly be in keeping with his morbidly jealous and self-centered character. Incidentally at a later date Hooke's invention of spiral springs for balance watches was rediscovered by Huyghens. Hooke then caused some of his new watches to be constructed, and published in cryptographical form the principle involved *Ut tensio sic vis*. It is a typical side-light on the whole affair that a resulting quarrel with Oldenburg culminated in Hooke's accusation of him as a "trafficker in intelligence."

It is well-known that shortly after the Restoration a distinguished group of natural philosophers in Oxford affiliated with a similar group in London to form a society, which was granted a charter by Charles II and is now the most famous of all scientific academies—the Royal Society. A small tract on capillary attraction, published by Hooke in 1661, soon attracted the attention of this society and on the 12th of November 1662, at the age of twenty-seven, he was appointed curator of experiments 'to furnish the society everyday they meet (once a week) with three or four considerable experiments, expecting no recompence till

the society get a stock enabling them to give it.' Hooke could not have obtained a better opportunity for a man of his temperament. He held this position for over forty years till his death in 1703. The society worked him hard and the Fellows, most of whom had but little experimental skill, were disconcerted if on any occasion he failed to be present at a meeting. Whatever apparatus he contrived 'for exhibiting any experiments before the Royal Society it was performed with the least embarrassment clearly and evidently.' The journal books record the trial by him of several hundreds of experiments, mostly new, by which 'facts multiplied, leading phenomena became prominent, laws began to emerge, and generalities to commence.'

On the 3rd of June 1663 Hooke was elected Fellow of the Royal Society and soon after Sir John Cutler founded for his benefit a lecture of £50 a year, leaving the number and subject of his discourses to the discretion of the Royal Society. In 1665 his office of curator was made perpetual with a salary of £30 and apartments in Gresham College where he resided for the rest of his life. He was also appointed Professor of Geometry in Gresham College.

At the beginning of September in the following year occurred the Great Fire of London and within a fortnight Hooke produced a comprehensive plan for the rebuilding of the city, the Lord Mayor preferring it to the model drawn up by the City Surveyor. Of the exact nature of Hooke's scheme we are ignorant, but it appears that he intended that the main streets from Leadenhall Corner to Newgate should lie in a straight line, the cross streets being at right angles. As a consequence of submitting this scheme Hooke was made City Surveyor 'to the no small hindrance of his Philosophical Disquisitions.' Unfortunately vested interests made it impossible for him to carry out his design. Nevertheless in this appointment Hooke succeeded in amassing several thousands of pounds, none of which he ever spent.

The registers of the Royal Society testify to the eagerness with which Hooke turned from one inquiry to another with brilliant but inconclusive results. Among other things he investigated the nature of the air, the relations of barometrical readings to

changes in the weather, and the law of falling bodies. Moreover he fixed the thermometrical zero at the freezing point of water and ascertained the number of vibrations corresponding to musical notes. This he explained to Pepys who thought Hooke's 'discourse in general mighty fine' but his pretension 'to tell how many strokes a fly makes with her wings a little too much refined.'

Hooke's impatient temperament and the multifarious duties imposed upon him prevented him from the sustained labour necessary to the production of large systematic works. It is therefore all the more remarkable that within three years of coming to London he should have found time to produce his one great treatise, the famous *Micrographia: or some Physiological Descriptions of Minute Bodies made by Magnifying Glasses with Observations and Inquiries thereupon.*

A new world, waiting to be explored for the first time, had been revealed early in the seventeenth century when Galileo made the first microscope, and characteristically Hooke was the first Englishman to chart this virgin territory with all the enthusiasm of a pioneer. Of the great classical microscopists—Leeuwenhoek, Swammerdamm, Malpighi, Grew and Hooke—Hooke was the most outstanding intellectually. He was soon aware of the immense complexity of Nature which the microscope revealed, and it is no exaggeration to say that his keen eye discerned 'a world in a grain of sand.'

The *Micrographia* is a collection of descriptions and engravings, many of great beauty, of minute bodies as viewed under a home-made microscope. Of scientific treatises produced by Englishmen in the seventeenth century it ranks second only to the *Principia.*

In the customary disingenuous preface to this great work, dedicated to Charles II, Hooke states that it is his object merely to describe and not to explain. He assures the reader that 'I have produced nothing here with intent to bind his understanding to an implicit consent; I am so far from that, that I desire him, not absolutely to rely upon the observations of my eyes, if he finds them contradicted by the future Ocular Experiments of

sober and impartial discoverers. . . . It is my hope as well as belief that these my labours will be no more comparable to the Productions of many other Natural Philosophers, who are everywhere busie about greater things; then my little objects are to be compar'd to the greater and more beautiful works of Nature, a Flea, a Mite or a Gnat to an Horse, an Elephant or a Lyon.'

Hooke begins by turning his microscope to the simplest objects and discovers that our perception of these things is completely transformed. His first observation is 'of the point of a sharp small needle' which he finds extremely blunt. He then examines the edge of a razor and remarks '. . . this Edge and piece of a Razor, if it had really been such as it appear'd through the Microscope, would scarcely have serv'd to cleave wood, much less to have cut off the hair of beards, unless it were after the manner that Lucan merrily related Charon to have made use of, when with a carpenter's axe he chop'd off the beard of a sage Philosopher, whose gravity he very cautiously fear'd would endanger the oversetting of his Wherry.'

And so Hooke proceeds to examine one small object after another and records his various observations of linen, of silk, of glass drops, of fiery sparks struck from flint or steel, of small diamants, of the schematisme or texture of cork and of the cells and pores of some other such frothy bodies, of the first principles of vegetation arising from putrefaction, of the curious texture of seaweeds, of stinging nettles, of the beard of a wild oat, of cowage and the itching operation of some bodies, of the scales of a sole, of the sting of a bee, of the teeth of a snail, of gnats and spiders, of the wandering mite—which he found travelling over his window at Oxford—of the crab-like insect, of the flea and of the louse. Of the flea he remarks 'The strength and beauty of this small creature, had it no other relation at all to man, would deserve a description,' and he accompanies his detailed description with a magnificent engraving.

In addition to these descriptions and despite Hooke's claim that it is not his object to manufacture hypotheses the *Micrographia* is full of 'ingenious ideas and singular anticipations.' First and foremost it contains Hooke's great discovery of the

cellular construction of plants. Indeed he was the first to introduce the word 'cell' into biological nomenclature. The *Micrographia* also contains a theory of light as a very short vibrative motion transverse to the straight line of propagation in a homogeneous medium. Moreover heat was defined as a property of a body arising from the motion or agitation of its parts, and the modern theory of combustion was partially anticipated eleven years before Mayow's similar hypothesis. Most important, though, of all the physical discoveries recorded in this great book was that of the interference of light. As a result of a large number of observations on the colours produced by light reflected from soap bubbles and other transparent films he recognized that the succession of colours which he observed was produced by the combination of a beam of light reflected from the upper surface of the film with a second beam which had penetrated the film and had been reflected from its lower surface. Unfortunately Hooke did not submit his observations to quantitative measurement and so the whole credit for the discovery has gone to Newton.

This neglect of mathematics by Hooke though obviously partly temperamental was also a definite scientific policy. In the preface to the *Micrographia* he wrote 'The truth is, the Science of Nature has already been made only a work of the Brain and the Fancy: it is now high time that it should return to the plainness and soundness of observations on material and obvious things. . . . If therefore the reader expects from me any infallible deductions or certainty of axioms, I am to say for myself that these stronger works of wit and imagination are above my weak abilities, or if they had not been so, I would not have made use of them in this present subject before me.'

Since certain authors of eminence[3] have recently appeared to adopt a sceptical attitude to mathematics it is relevant to recall the case of Hooke. We have already seen that his discoveries in the theory of light were overshadowed by those of Newton, because Newton adopted the mathematical method. That was in the period 1670–80. In the following decade an even more

[3] H. Dingle, 'Nature', 139, 784 (1937); J. Stark, *ibid.*, 141, 770 (1938).

important theory was to be developed by the natural philosopher with sufficient mathematical acumen—the Theory of Gravitation. Again Hooke was to be all but successful and Newton was to carry off the prize.

While at Oxford Hooke had been persuaded by the Savilian Professor, Seth Ward, to interest himself in Astronomy, and after settling in London he studied this 'Queen of the Sciences' with characteristic acuteness, originality and inconsequence. In particular he devoted much time and attention to the problem of accounting for the motions observed in the solar system and he was the first to state explicitly that he regarded this question as a problem in mechanics. Moreover with his unfailing scientific insight he developed his investigations along what were subsequently seen to be the right lines. Thus as early as 1666 he read a paper to the Royal Society explaining the distortion of uniform into curvilinear motion by 'a supervising attractive principle' but he was prevented from developing an exhaustive analysis by the *mathematical* difficulties of the problem. Two months later he demonstrated an experiment with a conical pendulum, which he likened to a planet going round the sun. The analogy is superficial but the experiment is striking.

We thus see that Hooke clearly realized that the paths of the planets were to be explained by a force constantly deflecting the planets from uniform motion. He surmised that gravity was the force concerned and he sought to determine the diminution of its effect with distance by weighing bodies hung at different heights from the top of Westminster Abbey, but, of course, without result. In 1670 he stated, apropos the mutual attraction of the heavenly bodies, that 'the true understanding of this principle will be the true perfection of astronomy.' He, like Kepler, regarded this force of attraction as a kind of radiation and so he was naturally led to consider the inverse square law, which he stated in 1678. Since Kepler had shown that the planets describe elliptical orbits about the sun as focus a clear cut *mathematical* problem now emerged. Does the inverse square law of attraction imply an elliptical orbit? This was the problem which Hooke was never able to solve and thereby surrendered to

Newton the entire credit for the Theory of Gravitation. By supreme irony it was a somewhat acrimonious correspondence with Hooke in 1679 on the conjectured paths of projectiles which finally induced Newton to resume his former thoughts concerning the moon. When the *Principia* was ultimately presented to the Royal Society in 1686 Hooke protested that Newton had plagiarized him. This claim of Hooke's evoked the scholium to the fourth proposition of the first book admitting Hooke's anticipation. Nevertheless the whole controversy was so distasteful to Newton that rather than endure any further attacks from Hooke, such as might arise in connection with the theory of light, he deliberately suppressed his *Optics* until 1704, the year after Hooke's death.

In thus recalling this notorious controversy we must remember that Newton, despite his consummate intellectual genius, often displayed an almost childish pettiness, meeting criticism by ungenerous attacks upon his opponents and by threatening to abandon his scientific work whenever he imagined that it was not properly appreciated by an ungrateful world.

Moreover, in all fairness to Hooke, we must not forget that his mind was warped by his appalling physique. One has to read but a page or two of his *Diary*, with its continual record of griping in the stomach, of sweating by night, of nightmares concerning 'viragoes and other strange phenomena,' of 'vagrant paines and witches' to realize what a titanic physical struggle he must have fought during the whole period of his intellectual activity.

Despite his infirmities Hooke was no recluse. As his diary reveals, he combined intellectual pleasures with those of a more worldly nature and he records indiscretions with complete frankness. His diary, unlike Pepys', has no literary significance, but in it we find many valuable details of daily life in a London which was springing to life again after the Great Fire.

His contemporary biographer Waller gives us the following pen picture. 'As to his person he was but despicable, being very crooked . . . This made him low of stature, though by his limbs he should have been moderately tall. He was always very pale and lean . . . with a meagre aspect, his eyes grey and full . . . his

Nose but thin . . . his Mouth meanly wide . . . his chin sharp and forehead large; his head of a middle size. He wore his own hair of a dark brown colour, very long and hanging neglected over his face, uncut and lank.

'He was of an active, restless, indefatigable genius . . . and always slept little . . . His temper was melancholy, mistrustful and jealous, which more increased upon him with his years. . . .

'From his youth he had been used to a collegiate, or rather monastick life, which might be some reason of his continuing to live so like an hermit or cynic too penuriously, when his circumstances . . . were very considerable.

'He always expressed a great veneration for the eternal and immense Cause of all beings . . . and was a frequent studier of the Holy Scriptures in the originals.'

In this context it is of interest to recall that in 1692 he delivered 'a curious discourse on the Tower of Babel.'

During the last few years of his life Hooke led a miserable existence, being much overrun with scurvy. He died on the 3rd of March 1703 and was buried at St. Helen's in London, 'all the members of the Royal Society then in town attending his body to the grave, paying the respect due to his extraordinary merit.'

To sum up in a concluding paragraph the achievements, so many and varied, of Robert Hooke is an almost impossible task. It is, however, as a master of *experiment* that he displayed his true genius. If, at the same time, he had devoted more attention to *mathematics* he might well have forestalled his great contemporary. Instead, however, to quote the admirable words of Sir Oliver Lodge[4]—'Fate has been a little unkind to Hooke in placing him so near to Newton; had he lived in an ordinary age he would undoubtedly have shone as a star of the first magnitude. With great ingenuity, remarkable scientific insight, and consummate experimental skill, he stands in many respects almost on a level with Galileo.'

Christ Church, Oxford.

[4] 'Pioneers of Science' (1926) p. 188.

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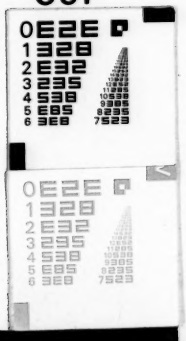
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